PROCEEDINGS

OF

THE AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

ELEVENTH MEETING,

HELD AT MONTREAL, CANADA EAST,

AUGUST, 1857.

CAMBRIDGE:
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JOSEPH LOVERING,

Permanent Secretary.

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AT THE

MONTREAL MEETING.

Prof. J. W. Bailey,* *President.*
Prof. Alexis Caswell, *Vice-President.*
Prof. Joseph Lovering, *Permanent Secretary.*
Prof. John Leconte, *General Secretary.*
Dr. A. L. Elwyn, *Treasurer.*

Standing Committee.

EX OFFICIO.

Prof. J. W. Bailey,*
Prof. Alexis Caswell,
Prof. Joseph Lovering,
Prof. John Leconte,
Dr. A. L. Elwyn,
Prof. James Hall,
Dr. B. A. Gould.

AS CHAIRMEN OF THE SECTIONAL COMMITTEES.

Prof. A. D. Bache,
Prof. J. D. Dana.

FROM THE ASSOCIATION AT LARGE.

Prof. J. W. Dawson,
Prof. Daniel Wilson,
Prof. Joseph Henry,
Prof. Benjamin Peirce,
Prof. Denison Olmsted,
Dr. P. N. Lynch.

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* Died before the Meeting.

Local Committee.

Sir WILLIAM E. LOGAN, *Chairman.*
A. N. RENNIE, *Secretary.*

THE MAYOR OF THE CITY,
THE PRESIDENT OF THE BOARD OF TRADE,
THE PRESIDENT OF THE NATURAL HISTORY SOCIETY,
Sir LOUIS LA FONTAINE,
Hon. P. J. O. CHAUVEAU,
Lieut. Col. MUNRO,
L. A. H. LATOUR,
H. H. WHITNEY,
Hon. Judge DAY,
L. H. HALTON, Esq.,
H. LYMAN, Esq.,
A. A. DONAN, Esq.,
Hon. P. D. BEAUJEU,
Hon. JAMES FERRIER,
Hon. GEORGE MOFFAT,
T. STERRY HUNT,
W. H. HINGSTON, M. D.,
D. KINNEAR, Esq.

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Gov. S. P. CHASE,
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Judge GEORGE HOADLEY, Jr.,
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Pres. J. W. ANDREWS,
Prof. JOSEPH HENRY,
Prof. J. D. DANA,
Prof. LOUIS AGASSIZ.

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Prof. A. D. BACHE,
Prof. JOSEPH HENRY,
Prof. J. H. ALEXANDER,
Prof. JOHN F. FRAZER,

Prof. Wolcott Gibbs,
Prof. Benjamin Peirce,
Prof. John LeConte,
Prof. W. B. Rogers,
Dr. J. H. Gibbon,
Dr. B. A. Gould, Jr.,
Prof. J. Lawrence Smith,
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Prof. J. D. Dana.

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Prof. Joseph Henry,
Prof. Benjamin Peirce,
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Prof. Denison Olmsted.

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Prof. Benjamin Peirce,
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Prof. J. F. Frazer,
Prof. William Chauvenet,
Pres. F. A. P. Barnard,
Prof. John LeConte,
Prof. W. M. Gillespie,
Prof. Francis M. Smith,
Prof. W. H. C. Bartlett,
Prof. Wolcott Gibbs,
Prof. Stephen Alexander,
Prof. Lewis R. Gibbes,
Prof. Joseph Winlock,
Prof. James Phillips,
Prof. William Ferrel,
Prof. Edward Hitchcock,
Prof. James D. Dana.

4. *Committee to Audit the Accounts of the Permanent Secretary and the Treasurer.*

Prof. DENISON OLMSTED,
Dr. B. A. GOULD.

5. *Committee on Rules and Regulations.*

Dr. B. A. GOULD,
Prof. A. D. BACHE,
Prof. JOSEPH LOVERING.

6. *Committee to act with the Standing Committee in Nomination of Officers for next Meeting.*

Section A.

Prof. E. N. HORSFORD,
Professor SMITH,
Prof. JAMES ROBB,
Dr. L. H. STEINER,

Section B.

Prof. B. SILLIMAN, Jr.,
Sir W. E. LOGAN,
Prof. EDWARD HITCHCOCK.

OFFICERS OF THE BALTIMORE MEETING.

Prof. JEFFRIES WYMAN, *President.*
Prof. JOHN E. HOLBROOK, *Vice-President.*
Prof. JOSEPH LOVERING, *Permanent Secretary.*
Prof. WILLIAM CHAUVENET, *General Secretary.*
Dr. A. L. ELWYN, *Treasurer.*

Standing Committee.

Prof. JEFFRIES WYMAN,
Prof. JOHN E. HOLBROOK,
Prof. JOSEPH LOVERING,
Prof. WILLIAM CHAUVENET,
Dr. A. L. ELWYN,
Prof. ALEXIS CASWELL,
Prof. JOHN LECONTE.

Local Committee.

Hon. THOMAS SWANN, *Chairman.*
Dr. LEWIS H. STEINER, *Secretary.*

Gen. JOHN SPEAR SMITH,
Hon. JOSHUA VANSANT,
J. F. MEREDITH,*
Dr. J. HANSON THOMAS,
WILLIAM H. YOUNG, Esq.,
Dr. CHRISTOPHER JOHNSTON,
AUGUSTUS J. ALBERT, Esq.,
WILLIAM MCKIM, Esq.,
GEO. W. BROWN, Esq.,
Rev. GEO. W. BURNAP, D. D.,
S. F. STREETER, Esq.,
Hon. S. O. HOFFMAN,
Dr. R. S. STUART,
Rev. JOHN G. MORRIS, D. D.,
JONATHAN MEREDITH, Esq.,
Dr. LEWIS H. STEINER,
WILLIAM P. SMITH.†

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* In place of Hon. John P. Kennedy, who declined.
† In place of Dr. Campbell Morfit, who declined.

MEETINGS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

Meeting.	Date.	Place.	President.	Vice-President.	General Secretary.	Permanent Secret'y.	Treasurer.
1st,	Sept. 20, 1848,	Philadelphia, Pa.,	W. C. Redfield, Esq.,		Prof. Walter R. Johnson,		Prof. J. Wyman.
2d,	Aug. 14, 1849,	Cambridge, Mass.,	Prof. Joseph Henry,		Prof. E. N. Horsford,		Dr. A. L. Elwyn.
3d,	March 12, 1850,	Charleston, S. C.,	Prof. A. D. Bache,*		Prof. L. R. Gibbes,*		Dr. St. J. Ravenel.*
4th,	Aug. 19, 1850,	New Haven, Ct.,	Prof. A. D. Bache,		E. C. Herrick, Esq.,		Dr. A. L. Elwyn.
5th,	May 5, 1851,	Cincinnati, Ohio,	Prof. A. D. Bache,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
6th,	Aug. 19, 1851,	Albany, N. Y.,	Prof. L. Agassiz,		Prof. W. B. Rogers,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
7th,	July 28, 1853,	Cleveland, Ohio,	Prof. Benj. Peirce,		Prof. J. D. Dana,	Prof. S. F. Baird,	Dr. A. L. Elwyn.
8th,	April 26, 1854,	Washington, D. C.,	Prof. J. D. Dana,		Prof. J. Lawrence Smith,	Prof. J. Lovering,	Dr. J. L. Leconte.*
9th,	Aug. 15, 1855,	Providence, R. I.,	Prof. John Torrey,		Prof. Wolcott Gibbs,	Prof. J. Lovering,	Dr. A. L. Elwyn.
10th,	Aug. 20, 1856,	Albany, N. Y.,	Prof. James Hall,		Dr. B. A. Gould, Jr.,	Prof. J. Lovering,	Dr. A. L. Elwyn.
11th,	Aug. 12, 1857,	Montreal, C. E.	Prof. J. W. Bailey,	Prof. Alexis Caswell.	Prof. John Leconte,	Prof. J. Lovering,	Dr. A. L. Elwyn.

* In the absence of the regular officer.

CONSTITUTION OF THE ASSOCIATION.*

OBJECTS.

THE Association shall be called "The American Association for the Advancement of Science." The objects of the Association are, by periodical and migratory meetings, to promote intercourse between those who are cultivating science in different parts of the United States; to give a stronger and more general impulse, and a more systematic direction, to scientific research in our country; and to procure for the labors of scientific men increased facilities and a wider usefulness.

MEMBERS.

RULE 1. Members of scientific societies or learned bodies having in view any of the objects of this Association, and publishing transactions, shall be considered members on subscribing these rules.

RULE 2. Collegiate professors, also civil engineers and architects who have been employed in the construction or superintendence of public works, may become members on subscribing these rules.

RULE 3. Persons not embraced in the above provisions may become members of the Association upon recommendation in

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* Adopted August 25, 1856, and ordered to go into effect at the opening of the Montreal Meeting.

writing by two members, nomination by the Standing Committee, and election by a majority of the members present.

OFFICERS.

RULE 4. The officers of the Association shall be a President, Vice-President, General Secretary, Permanent Secretary, and Treasurer. The President, Vice-President, General Secretary, and Treasurer, shall be elected at each meeting for the following one; — the three first-named officers not to be reëligible for the next two meetings, and the Treasurer to be reëligible as long as the Association may desire. The Permanent Secretary shall be elected at each second meeting, and also be reëligible as long as the Association may desire.

MEETINGS.

RULE 5. The Association shall meet, at such intervals as it may determine, for one week or longer — the time and place of each meeting being determined by a vote of the Association at the previous meeting; and the arrangements for it shall be intrusted to the officers and the Local Committee.

STANDING COMMITTEE.

RULE 6. There shall be a Standing Committee, to consist of the President, Vice-President, Secretaries, and Treasurer of the Association, the officers of the preceding year, the permanent chairmen of the Sectional Committees, after these shall have been organized, and six members present from the Association at large who shall have attended any of the previous meetings, to be elected upon open nomination by ballot on the first assembling of the Association. A majority of the whole number of votes cast to elect. The General Secretary shall be Secretary of the Standing Committee.

The duties of the Standing Committee shall be, —

1. To assign papers to the respective sections.

2. To arrange the scientific business of the general meetings, to suggest topics and arrange the programmes for the evening meetings.

3. To suggest to the Association the place and time of the next meeting.

4. To examine, and, if necessary, to exclude papers.

5. To suggest to the Association subjects for scientific reports and researches.

6. To appoint the Local Committee.

7. To have the general direction of publications.

8. To manage any other general business of the Association during the session, and during the interval between it and the next meeting.

9. In conjunction with four from each Section, to be elected by the sections for the purpose, to make nominations of officers of the Association for the following meeting.

10. To nominate persons for admission to membership.

11. Before adjourning, to decide which papers, discussions, or other proceedings shall be published.

SECTIONS.

RULE 7. The Association shall be divided into two Sections, and as many sub-Sections as may be necessary for the scientific business, the manner of division to be determined by the Standing Committee of the Association. The two Sections may meet as one.

SECTIONAL OFFICERS AND COMMITTEES.

RULE 8. On the first assembling of the Section, the members shall elect upon open nomination a permanent chairman and secretary, also three other members, to constitute, with these officers, a Sectional Committee.

The Section shall appoint, from day to day, a chairman to preside over its meetings.

RULE 9. It shall be the duty of the Sectional Committee of each Section to arrange and direct the proceedings in their Section; to ascertain what communications are offered; to assign the order in which these communications shall appear, and the amount of time which each shall occupy.

The Sectional Committees may likewise recommend subjects for systematic investigation by members willing to undertake the researches, and to present their results at the next meeting.

The Sectional Committees may likewise recommend reports on particular topics and departments of science, to be drawn up as occasion permits, by competent persons, and presented at subsequent meetings.

REPORTS OF PROCEEDINGS.

RULE 10. Whenever practicable, the proceedings shall be reported by professional reporters or stenographers, whose reports are to be revised by the secretaries before they appear in print.

PAPERS AND COMMUNICATIONS.

RULE 11. No paper shall be placed in the programme, unless admitted by the Sectional Committee; nor shall any be read, unless an abstract of it has been previously presented to the Secretary of the Section, who shall furnish to the chairman the titles of papers of which abstracts have been received.

RULE 12. The author of any paper or communication shall be at liberty to retain his right of property therein, provided he declare such to be his wish before presenting it to the Association.

RULE 13. Copies of all communications, made either to the General Association or to the Sections, must be furnished by the authors; otherwise only the titles or abstracts shall appear in the published proceedings.

RULE 14. All papers, either at the general or in the sectional meetings, shall be read, as far as practicable, in the order in which they are entered upon the books of the Association; except that those which may be entered by a member of the Standing Committee of the Association shall be liable to postponement by the proper Sectional Committee.

RULE 15. If any communication be not ready at the assigned time, it shall be dropped to the bottom of the list, and shall not be entitled to take precedence of any subsequent communication.

RULE 16. No exchanges shall be made between members without authority of the respective Sectional Committees.

GENERAL AND EVENING MEETINGS.

RULE 17. The Standing Committee shall appoint any general meeting which the objects and interests of the Association may call for, and the evenings shall, as a rule, be reserved for general meetings of the Association.

These general meetings may, when convened for that purpose, give their attention to any topics of science which would otherwise come before the Sections.

It shall be a part of the business of these general meetings to receive the Address of the President of the last meeting; to hear such reports on scientific subjects as, from their general importance and interests, the Standing Committee shall select; also to receive from the chairmen of the Sections abstracts of the proceedings of their respective Sections; and to listen to communications and lectures explanatory of new and important discoveries and researches in science, and new inventions and processes in the arts.

ORDER OF PROCEEDINGS IN ORGANIZING A MEETING.

RULE 18. The Association shall be called to order by the President of the preceding meeting, and this officer having resigned the chair to the president elect, the General Secretary shall then report the number of papers relating to each department which have been registered, and the Association consider the most eligible distribution into Sections, when it shall proceed to the election of the additional members of the Standing Committee in the manner before described; the meeting shall then adjourn, and the Standing Committee, having divided the Association into Sections as directed, shall allot to each its place of meeting for the Session. The Sections shall then organize by electing their officers and their representatives in the Nominating Committee, and shall proceed to business.

PERMANENT SECRETARY.

RULE 19. It shall be the duty of the Permanent Secretary to notify members who are in arrears, to provide the necessary stationery and suitable books for the list of members and titles of papers, minutes of the general and sectional meetings, and for other purposes indicated in the rules, and to execute such other duties as may be directed by the Standing Committee or by the Association.

The Permanent Secretary shall make a report annually to the Standing Committee, at its first meeting, to be laid before the Association, of the business of which he has had charge since its last meeting.

All members are particularly desired to forward to the Permanent Secretary, so as to be received before the day appointed for the Association to convene, complete titles of all the papers which they expect to present during its meeting, with an estimate of the time required for reading each, and such abstracts of their contents as may give a general idea of their nature.

Whenever the Permanent Secretary notices any error of fact or unnecessary repetition, or any other important defect in the papers communicated for publication in the proceedings of the Association, he is authorized to commit the same to the author, or to the proper sub-committee of the Standing Committee for correction.

LOCAL COMMITTEE.

Rule 20. The Local Committee shall be appointed from among members residing at or near the place of meeting for the ensuing year; and it shall be the duty of the Local Committee, assisted by the officers, to make arrangements and the necessary announcements for the meeting.

The Secretary of the Local Committee shall issue a circular in regard to the time and place of meetings, and other particulars, at least one month before each meeting.

SUBSCRIPTIONS.

Rule 21. The amount of the subscription, at each meeting, of each member of the Association shall be two dollars, and one dollar in addition shall entitle him to a copy of the proceedings of the annual meeting. These subscriptions shall be received by the Permanent Secretary, who shall pay them over, after the meeting, to the Treasurer.

No person shall be considered a member of the Association until the subscription for the meeting at which he is elected has been paid.

Rule 22. The names of all persons two years in arrears for annual dues shall be erased from the list of members; provided that two notices of indebtedness, at an interval of at least three months, shall have been previously given.

ACCOUNTS.

Rule 23. The accounts of the Association shall be audited annually, by auditors appointed at each meeting.

ALTERATIONS OF THE CONSTITUTION.

Rule 24. No article of this constitution shall be altered, or amended, or set aside, without the concurrence of three fourths of the members present, and unless notice of the proposed change shall have been given at the preceding annual meeting.

RESOLUTIONS

OF A

PERMANENT AND PROSPECTIVE CHARACTER,

ADOPTED AUGUST 19, 1857.

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1. No appointment may be made in behalf of the Association, and no invitation given or accepted, except by vote of the Association or its Standing Committee.

2. The General Secretary shall transmit to the Permanent Secretary for the files, within two weeks after the adjournment of every meeting, a record of the proceedings of the Association and the votes of the Standing Committee. He shall also daily, during the meetings, provide the chairmen of the two sectional committees with lists of the papers assigned to their Sections by the Standing Committee.

3. All printing for the Association shall be superintended by the Permanent Secretary, who is authorized to employ a clerk for that especial purpose.

4. The Permanent Secretary is authorized to put the proceedings of the meetings to press one month after the adjournment of the Association. Papers which have not been received at that time may be published only by title. No notice of articles not approved shall be taken in the published proceedings.

5. The Permanent Chairmen of the Sections are to be considered their organs of communication with the Standing Committee.

6. It shall be the duty of the Secretaries of the two Sections to receive copies of the papers read in their Sections, all subsections

included, and to furnish them to the Permanent Secretary at the close of the meeting.

7. The Sectional Committees shall meet not later than 9 A. M. daily during the meetings of the Association, to arrange the programmes of their respective Sections, including all subsections, for the following day. No paper shall be placed upon these programmes which shall not have been assigned to the Section by the Standing Committee. The programmes are to be furnished to the Permanent Secretary not later than 11 A. M.

8. During the meetings of the Association the Standing Committee shall meet daily, Sundays excepted, at 9 A. M., and the Sections be called to order at 10 A. M., unless otherwise ordered. The Standing Committee shall also meet on the evening preceding the first assembling of the Association at each annual meeting, to arrange for the business of the first day, and on this occasion three shall form a quorum.

9. Associate members may be admitted for one, two, or three years, as they shall choose at the time of admission, — to be elected in the same way as permanent members, and to pay the same dues. They shall have all the social and scientific privileges of members, without taking part in the business.

10. No member may take part in the organization and business arrangements of both the sections.

MEMBERS

OF THE

AMERICAN ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

NOTE.—Names of deceased members are marked with an asterisk (*); and those of members who, in 1840, formed the original "Association of American Geologists," are in small capitals. The figure at the end of each name refers to the meeting at which the election took place.

A.

Abbott, Gorham D., New York [7].
Abert, Col. J. J., Washington, D. C. [1].
*Adams, Prof. C. B., Amherst, Massachusetts [1].
Adamson, J. C., New York [7].
Agassiz, Prof. Louis, Cambridge, Massachusetts [1].
Alexander, Prof. Stephen, Princeton, New Jersey [1].
Allen, Prof. E. A. H., New Bedford, Massachusetts [6].
Allen, George N., Oberlin, Ohio [5].
Allen, John H., Oxford, Maryland [6].
Allen, Nathaniel T., West Newton, Massachusetts [10].
Allen, R. L., M. D., Saratoga Springs, New York [10].
Allen, Zachariah, Esq., Providence [1].
Allston, R. F. W., Esq., Georgetown, South Carolina [3].
Allyn, Rev. Robert E., Greenwich, Rhode Island [9].
Ames, Bernice D., Fort Edward, New York [10].

*Ames, M. P., Esq., Springfield, Massachusetts [1].
Amory, Jonathan, Jamaica Plains, Massachusetts [8].
Anderson, Pres. M. B., Rochester, New York [10].
Andrews, Alonzo, Lewiston, Maine [7].
Andrews, Dr. E. H., Charlotte, North Carolina [3].
Andrews, Prof. E. B., Marietta, Ohio [7].
Andrews, Israel W., Marietta, Ohio [11].
Anthony, Charles H., Esq., Albany [6].
Anthony, Henry, Providence [9].
Anthony, J. G., Esq., Cincinnati, Ohio [1].
Appleton, Nathan, Esq., Boston [1].
Appleton, Thomas G., Boston [8].
Arden, Thomas B., Garrison's P. O., Putnam Co., New York [7].
Armour, A. H., Toronto, Canada [10].
Armsby, Prof. J. H., Albany [6].
Astrop, R. F., Crichton's Store, Burns Co., Virginia [7].
Atterbury, Rev. John G., New Albany, Indiana [11].
Austin, Samuel, Providence [9].

B.

Baby, G., Montreal [11].
Bache, Prof. Alexander D., Washington, D. C. [1].
Bache, Dr. Franklin, Philadelphia [1].
Bacon, Dr. John, Jr., Boston [1].
Bacon, William, Richmond, Berkshire Co., Massachusetts [7].
Bagg, Moses M., Utica, New York [4].
*Bailey, Prof. J. W., West Point, New York [1].
Baird, Prof. S. F., Washington, D. C. [1].
Baldwin, F. H., Waverly, New York [10].
Barber, Edgar A., Albany, New York [10].
Barlow, Thomas, Canastota, New York [7].
Barnard, Pres. F. A. P., Oxford, Mississippi [7].
Barnes, Capt. James, Springfield, Massachusetts [5].
Barnston, Dr. James, Montreal, Canada [10].
Barratt, Dr. J. P., Barrattsville, South Carolina [3].
Barrows, George B., Fryeburg, Maine [7].
Bartlett, J. R., Providence [8].
Bartlett, Prof. W. H. C., West Point, New York [9].

Barton, Dr. E. H., New Orleans [9].
Bassnett, Thomas, Ottawa, Illinois [8].
Batchelder, J. M., Cambridge, Massachusetts [8].
Baudry, J. H., Montreal [11].
Baylis, James, Montreal [11].
Beadle, Dr. E. L., New York [1].
Beadle, Rev. E. R., Hartford, Connecticut [10].
Bean, Sidney A., Waukesha, Wisconsin [9].
Beck, Dr. C. F., Philadelphia [1].
*Beck, Prof. Lewis C., New Brunswick, New Jersey [1].
*Beck, Dr. T. Romeyn, Albany [1].
Bell, Samuel N., Manchester, New Hampshire [7].
Belle, Charles E., Montreal [11].
Benedict, Erastus C., New York [10].
Benedict, F. N., Parisippany, New Jersey [1].
Benedict, Dr. N. B., New Orleans [10].
Bent, Silas, U. S. N., New York [10].
Berezy, William, Daillehaut, Canada East, [11].
Bernard, Dr. A., Montreal [11].
Bidwell, Walter H., New York [11].
Bigelow, Artemas, Newark, New Jersey [9].
Billings, E., Montreal [11].
*Binney, Dr. Amos, Boston [1].
Binney, Amos, Esq., Boston [9].
*Binney, John, Esq., Boston [3].
Blackie, Dr. George S., Nashville, Tennessee [10].
Blackmarr, Rev. Henry, Rochester, New York [11].
Blake, Rev. C. M., Chili [11].
Blake, Eli W., New Haven, Connecticut [1].
Blake, J. R., LaGrange, Tennessee [10].
Blake, William P., Esq., Washington, D. C. [2].
*Blanding, Dr. William, Rhode Island [1].
Blatchford, Dr. Thomas W., Troy, New York [6].
Bolton, James, Richmond, Virginia [10].
*Bomford, Col. George, Washington, D. C. [1].
Bonar, Rev. J. B., Montreal [11].
Bond, George P., Esq., Cambridge, Massachusetts [2].
Bond, William C., Esq., Cambridge, Massachusetts [2].

Bonnycastle, Sir Charles, Montreal, Canada [1].
Borland, J. N., M. D., Boston [9].
Botta, Prof. Vincenzo, New York [9].
Bouve, Thomas T., Boston [1].
Bowditch, Henry J., M. D., Boston [2].
Boyden, Uriah A., Esq., Boston [2].
Boynton, John F., Esq., Chicago, New York [4].
Bradford, James C., M. D., Elyria, Ohio [11].
Bradford, George W., Homer, New York [10].
Brainerd, Prof. Jehu, Cleveland, Ohio [5].
Braithwaite, Rev. Joseph, Chambly, Canada West [11].
Brant, James R., New York [9].
Breevort, J. Carson, Brooklyn, New York [1].
Brewer, Fisk P., New Haven [11].
Briston, William, Montreal [11].
Brocklesby, Prof. John, Hartford, Connecticut [4].
Brooks, Rev. Charles, Boston [9].
Bross, William, Chicago, Illinois [7].
Brown, Andrew, Esq., Natchez, Mississippi [1].
Brown, John C., Esq., Providence [9].
Brown, Richard, Esq., Sydney, Cape Breton [1].
Brown, Robert, Jr., New Haven [11].
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Brush, George S., Montreal [11].
Buchanan, Robert, Esq., Cincinnati, Ohio [2].
Buell, David, Jr., Troy, New York [6].
Bulkley, John W., Brooklyn, New York [10].
Bullard, Edward F., Waterford, New York [10].
*Burnett, Waldo I., Esq., Boston [1].
Burton, Dr. C. V. W., Lansingburg, New York [6].
Busher, James, Worcester, Massachusetts [9].
Butler, Hon. Thomas B., Norwalk, Connecticut [10].

C.

Cabell, Prof. James L., University of Virginia [6].
Cady, Rev. Calvin B., Alburgh, Vermont [11].
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Campbell, A. D. Montreal [11].
Campbell, Prof. George W., Montreal [11].
Campbell, W. D., Quebec [11].
Cartier, Hon. G. E., Montreal [11].
Carley, S. T., Esq., Cincinnati, Ohio [5].
*Carpenter, Thornton, Camden, South Carolina [7].
*Carpenter, Dr. William M., New Orleans [1].
Carr, E. S., Albany [9].
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Cassells, Prof. J. L., Cleveland, Ohio [7].
Caswell, Prof. Alexis, Providence [2].
Cavert, Michael P., Amsterdam, New York [10].
Chadbourne, Prof. P. A., Williamstown, Massachusetts [10].
Chamberlain, Frank, Albany [10].
Chamberlin, B. C., Montreal [11].
Channing, William F., Esq., Boston [2].
Chapin, L. C., New Haven, Connecticut [11].
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Nichols, Dr. James R., Haverhill, Massachusetts [7].
Nichols, Prof. John A., New York [10].
*Nicollet, J. N., Esq., Washington, D. C. [1].
North, Prof. Edward, Clinton, New York [11].
Norton, Rev. Niram, Fredonia, New York [8].
*Norton, Prof. J. P., New Haven, Connecticut [1].
Norton, Prof. W. A., New Haven, Connecticut [6].
Nott, Dr. J. C., Mobile, Alabama [3].

O.

*Oakes, William, Esq., Ipswich, Massachusetts [1].
O'Callaghan, E. B., M. D., Albany [10].
Olcott, Thomas W., Esq., Albany [2].
O'Leary, Prof. Charles, Emmetsburg, Maryland [10].
Oliver, James Edward, Lynn, Massachusetts [7].

*Olmsted, Alexander F., New Haven, Connecticut [4].
Olmsted, Charles H., Esq., E. Hartford, Connecticut [1].
Olmsted, Prof. D., New Haven, Connecticut [1].
*Olmsted, Denison, Jr., Esq., New Haven, Connecticut [1].
Olney, Stephen T., Providence [9].
Opdyke, George, New York [8].
Ordway, John M., Roxbury, Massachusetts [9].
Ormiston, Rev. William, Hamilton, Canada [10].
Osborn, A., Esq., Albany, New York [1].
Osborn, H. S., Liberty, Bedford Co., Virginia [10].
Osgood, Rev. Samuel, D. D., New York [8].
Osten Sacken, Baron R. von, Washington, D. C. [10].
Otis, George A., Jr., Springfield, Massachusetts [10].

P.

Painter, Minshall, Lima, Delaware Co., Pennsylvania [7].
Palmer, Charles H., Portage Lake, Michigan [6].
Paradis, H. C. A., M. D., Montreal [11].
Parker, Hon. Amasa J., Albany [6].
Parker, Rev. Henry E., Concord, New Hampshire [11].
Parker, Prof. William H., Middlebury, Vermont [10].
*Parkman, Dr. Samuel, Boston [1].
Parry, Dr. Charles C., Davenport, Iowa [6].
Parsons, Dr. Charles H., Providence [9].
Parsons, Dr. Usher, Providence [9].
Parvin, Theodore S., Muscatine, Iowa [7].
Paton, George, Galt, Canada West [11].
Paton, John, Toronto [11].
Patterson, Prof. J. W., Hanover, New Hampshire [11].
Peale, Titian R., Esq., Washington, D. C. [1].
Peck, Lieut. G. W., U. S. T. E., New York [9].
Peirce, Prof. Benjamin, Cambridge, Massachusetts [1].
Peirce, James W., Cambridge, Massachusetts [11].
Pendleton, A. G., U. S. N., Washington, D. C. [7].
Perkins, Prof. G. R., Utica, New York [1].
Perkins, J. A., Montreal [11].
*Perry, Com. M. C., New York [10].
Peters, Dr. C. H. F., Washington, D. C. [9].

Phelps, Philip, Jr., Hastings, New York [10].
Phelps, Prof. William F., Trenton, New Jersey [11].
Pickering, Dr. Charles, Boston [1].
Pierce, Prof. William M., Hadley, Massachusetts [10].
Pigott, A. Snowden, Baltimore [10].
Pitcher, Dr. Zina, Detroit, Michigan [1].
Pitman, Benn, Cincinnati [10].
Plant, I. C., Esq., Macon, Georgia [3].
*Plumb, Dr. Ovid, Salisbury, Connecticut [9].
Porter, Prof. John A., New Haven, Connecticut [4].
Potter, Rev. Dr. H. B., Albany [6].
Pourtales, L. F., Esq., Washington, D. C. [1].
Powers, A. E., Esq., Lansingburg, New York [1].
Prentice, E. P., Esq., Albany [2].
Prescott, Dr. William, Concord, New Hampshire [1].
Priest, Rev. J. A., Homer, New York [10].
Prince, W. R., Flushing, Long Island [10].
Prout, H. A., M. D., St. Louis, Missouri [7].
Pruyn, J. V. L., Esq., Albany [1].
Pruyn, Robert H., Albany [10].
Putnam, F. W., Salem, Massachusetts [10].
Pybos, Benjamin, Tuscumbia, Alabama [7].
Pynchon, Prof. Thomas R., Hartford, Connecticut [11].

Q.

Quincy, Edmund, Jr., Dedham, Massachusetts [11].

R.

Ramsay, Andrew C., London [11].
Ramsay, J. K., Montreal [11].
Rankin, Robert G., New York [10].
Rauch, John H., M. D., Burlington, Iowa [11].
Ravenel, Dr. Edmund, Charleston, South Carolina [1].
Ray, Dr. Isaac, Providence [9].
Read, D. B., Toronto [11].
Redfield, Charles B., Albany [11].
Redfield, John H., Esq., New York [1].

*Redfield, William C., Esq., New York [1].
Reid, D. B., London [10].
Renwick, Prof. James, New York [1].
Resor, Jacob, Cincinnati, Ohio [8].
Reuben, Levi, New York [10].
Rice, Clinton, New York [7].
Rice, Henry, North Attleborough, Massachusetts [7].
Rice, W. A., Albany [10].
Richards, Z., Washington, D. C. [7].
Richards, Rev. William C., Providence [9].
Riddell, Dr. John L., New Orleans [1].
Riddell, William P., New Orleans [7].
Ripley, Hezekiah W., Esq., Harlan, New York [6].
Ritchie, E. S., Boston [10].
Robb, Prof. James, M. D., Fredericton, New Brunswick [4].
Robertson, Thomas D., Rockford, Illinois [10].
Robertson, Rev. W. H. C., Stamford, Canada [11].
Roberts, W. M., Carlisle, Pennsylvania [5].
Robinson, W. B., Toronto [11].
Rockwell, Alfred P., New Haven, Connecticut [10].
Rockwell, John, Chicago, Illinois [11].
Rockwell, John A., Norwich, Connecticut [10].
Rodman, William M., Providence [9].
Rogers, Prof. Fairman, Philadelphia [11].
*Rogers, Prof. James B., Philadelphia [1].
ROGERS, Prof. ROBERT E., Philadelphia [1].
Rogers, Prof. W. B., Boston [1].
Rogers, William F., Philadelphia [10].
Rood, Ogden N., New Haven, Connecticut [7].
Roome, Martin R., New York [10].
Roosevelt, Clinton, New York [11].
Rose, Henry, Montreal [11].
Roy, Euclide, Montreal [11].
Ruggles, Prof. William, Washington, D. C. [8].
Runkle, J. D., Esq., Dedham, Massachusetts [2].
Russell, Andrew, Toronto [11].
Russell, Archibald, New York [11].

Ruttan, Dr. Allan, Newburgh, Canada West [11].
Ryan, Thomas, Montreal [11].
Ryerson, Rev. E., Montreal [11].

S.

Safford, Prof. J. M., Lebanon, Tennessee [6].
Sager, Prof. Abraham, Ann Arbor, Michigan [6].
Sanderson, Rev. J. E., Montreal [11].
Sanford, R. K., Riga, New York [7].
Sargent, Rufus, Auburn, New York [10].
Savage, Thomas S., Pass Christian, Mississippi [10].
Saville, Henry M., Syracuse, New York [10].
Saxton, Joseph D., Esq., Washington, D. C. [1].
Scarborough, Rev. George, Owensburg, Kentucky [2].
Schaeffer, Prof. George C., Washington, D. C. [1].
Schanck, Prof. J. Stillwell, Princeton, New Jersey [4].
Schnee, Alexander, Madison, Wisconsin [10].
Schoolcraft, Henry R., Washington, D. C. [7].
Schott, Arthur C. V., Washington, D. C. [8].
Schott, Charles A., Washington, D. C. [8].
Scott, Rev. Joseph, Dunham, Canada East [11].
Scott, Rev. William, Peterborough, Canada West [11].
Selden, George M., Esq., Troy, New York [6].
Seemann, Berthold, Ph. D., London [11].
Seropyan, Christopher D., New York [10].
Sessions, Rev. John, Albany [6].
Sestini, Prof. Benedict, Washington, D. C. [8].
Seward, Hon. William H., Auburn, New York [1].
Seymour, M. H., Montreal [11].
Shaefer, P. W., Pottsville, Pennsylvania [4].
Shaffer, David H., Cincinnati, Ohio [7].
Shane, J. D., Lexington, Kentucky [7].
Shaw, Edward, Washington, D. C. [9].
Sheldon, D. H., Racine, Wisconsin [10].
Shelton, E. E., Montreal [11].
Shepard, Prof. C. U., New Haven, Connecticut [4].
Sheppard, William, Drommondville, Canada [11].
Sherwin, Thomas, Dedham, Massachusetts [11].

Shotwell, Samuel L., Macedon, New York [11].
Shumard, B. F., M. D., St. Louis, Missouri [7].
Sias, Solomon, Fort Edwards, New York [10].
Sill, Hon. Elisha N., Cuyahoga Falls, Ohio [6].
Silliman, Prof. Benjamin, New Haven, Connecticut [1].
Silliman, Prof. Benjamin, Jr., New Haven, Connecticut [1].
Skilton, Dr. Avery J., Troy, New York [6].
Skinner, George W., M. D., Little Falls, New York [10].
Smallwood, Charles, M. D., St. Martin, Isle Jesus, Canada East [7].
Smith, Pres. Augustus W., Middletown, Connecticut [4].
Smith, Capt. E. K., U. S. A. [8].
Smith, Prof. Francis H., Charlottesville, Virginia [9].
Smith, George, Upper Darby, Delaware Co., Pennsylvania [7].
Smith, J. Bryant, M. D., New York [7].
Smith, James Y., Providence [9].
*Smith, J. V., Esq., Cincinnati, Ohio [5].
Smith, Prof. J. Lawrence, Louisville [1].
Smith, John, Montreal [11].
Smith, Dr. Lyndon A., Newark, New Jersey [9].
Smith, Metcalf J., M'Granville, New York [10].
Smith, Sanderson, New York [9].
Smith, Hamilton L., Gambier, Ohio [5].
Smith, Spencer, St. Louis, Missouri [11].
Snell, Prof. Eben S., Amherst, Massachusetts [2].
Snodgrass, Rev. William, Montreal [11].
Snow, Charles B., Washington, D. C. [8].
Snow, Edwin M., M. D., Providence [9].
Sola, Rev. Dr. A., Montreal [11].
Sparks, Jared, Cambridge, Massachusetts [2].
Spear, C. V., Pittsfield, Massachusetts [10].
Spink, William, Toronto [11].
Sprague, Charles Hill, Malden, Massachusetts [7].
Sprague, Daniel J., South Orange, New Jersey [11].
Sprague, Rev. Daniel G., South Orange, New Jersey [11].
Stanard, Benjamin A., Esq., Cleveland, Ohio [6].
Starr, William, Ceresco, Wisconsin [10].
Stearns, Eben S., Albany [10].
Stearns, Josiah A., Boston [10].

Stebbins, Rev. Rufus P., Cambridge, Massachusetts [2].
Steele, Samuel, Albany [10].
Steiner, Dr. Lewis H., Baltimore, Maryland [7].
Stetson, Charles, Cincinnati, Ohio [4].
Stevens, Prof. M. C., Richmond, Indiana [9].
Stevens, Robert P., M. D., North Egremont, Massachusetts [7].
Stewart, Prof. William M., Clarksville, Tennessee [7].
Stillman, Thomas B., New York [8].
Stone, Rev. Edwin M., Providence [9].
Storer, Dr. D. H., Boston [1].
Street, Alfred B., Albany [10].
Streeter, S. F., Baltimore, Maryland [11].
Stuart, Prof. A. P. S., Acadia, Nova Scotia [11].
Sturtevant, Pres. J. M., Jacksonville, Illinois [10].
Suckley, Dr. George, U. S. A. [9].
Sullivant, William S., Columbus, Ohio [7].
Sutherland, Prof. William, Montreal, Canada [6].
Swallow, G. C., Columbia, Missouri [10].
Swan, Gen. Lansing B., Rochester, New York [8].
Sweeney, Peter, Buffalo, New York [10].
Swinburne, John, Albany [6].

T.

Tabor, Azor, Albany [6].
Talcott, Andrew, Cincinnati, Ohio [7].
*Tallmadge, Hon. James, New York [1].
Tappan, Chancellor H. L., Ann Arbor, Michigan [10].
Tatlock, Prof. John, Williamstown, Massachusetts [10].
Tatum, Joel H., Baltimore, Maryland [10].
Tayler, John W., Wampsville, New York [10].
Taylor, George W., Albany [10].
Taylor, Dr. Julius S., Carrolton, Montgomery Co., Ohio [1].
Taylor, Morse K., M. D., Galesburg, Knox Co., Illinois [7].
*TAYLOR, RICHARD C., Esq., Philadelphia [1].
Taylor, J. W., Montreal [11].
Taylor, Rev. William, Montreal [11].
Taylor, Thomas M., Montreal [11].
Tefft, Thomas A., New York [9].

*Teschemacher, J. E., Esq., Boston [1].
Tevis, Robert C., Esq., Shelbyville, Kentucky [5].
Thickstun, I. F., Meadville, Pennsylvania [11].
Thomas, Richard, Montreal [11].
Thomas, William A., Irvington, New York [10].
Thompson, Dr. Alexander, Aurora, New York [6].
Thompson, Aaron R., New York [1].
Thompson, John A., M. D., Cayuga, New York [10].
Thompson, Dr. J. W., Wilmington, Delaware [9].
Thompson, John Edgar, Esq., Philadelphia [1].
*Thompson, Rev. Z., Burlington, Vermont [1].
Thomson, Rev. John, New York [11].
Thorn, James, M. D., Troy, New York [10].
Thurber, George, Esq., New York [1].
Thurber, Isaac, Providence [9].
Tobey, Dr. Samuel B., Providence [9].
Tolderoy, James B., M. D., Fredericton, New Brunswick [11].
Torrey, Dr. John, New York [1].
Torrey, Prof. Joseph, Burlington, Vermont [2].
Totten, Gen. J. G., U. S. A., Washington, D. C. [1].
Town, Salem, Aurora, New York [7].
Townsend, Hon. Franklin, Albany [4].
Townsend, Dr. Howard, Albany [10].
*Townsend, John K., Esq., Philadelphia [1].
Townsend, Robert, Albany [9].
Treadwell, C. P., L'Original, Canada West [11].
*Troost, Dr. Gerard, Nashville, Tennessee [1].
Trowbridge, Prof. W. P., Ann Arbor, Michigan [10].
Trudeau, Alexis, Montreal [11].
Truesdell, Samuel, New York [10].
Trumbull, James H., Esq., Hartford, Connecticut [4].
*Tuomey, Prof. M., Tuscaloosa, Alabama [1].
Turnbull, Lawrence, Philadelphia [10].
Turner, William W., Washington, D. C. [7].
Tuthill, Franklin, M. D., New York [8].
*Tyler, Rev. Edward R., New Haven, Connecticut [1].
Tyler, Ransom H., Fulton, New York [10].
Tyler, Robert S., Montreal [11].

V.

Vail, Prof. Hugh, Haverford, Pennsylvania [8].
Vancleve, John W., Dayton, Ohio [1].
Van Duzee, William S., M. D., Buffalo, New York, [7].
Van Derpool, S. Oakley, Albany, New York [9].
Van Pelt, Wm., M. D., Williamsville, Erie Co., New York [7].
*VANUXEM, LARDNER, Esq., Bristol, Pennsylvania [1].
Van Vleck, J. M., Middletown, Connecticut [9].
Vaughan, Daniel, Esq., Cincinnati, Ohio [5].
Vaux, William S., Esq., Philadelphia [1].
Verreau, H. A. B., Montreal, Canada [10].

W.

Wadsworth, Charles F., Geneseo, New York [11].
Wadsworth, James S., Esq., Genesee, New York [2].
Wagner, Tobias, Philadelphia [9].
Walker, Rev. James B., Mansfield, Ohio [7].
Walker, Joseph, Oxford, New York [10].
*Walker, Sears C., Esq., Washington, D. C. [1].
*Walker, Hon. Timothy, Cincinnati, Ohio [4].
Wallbridge, T. C., Toronto [11].
Walling, Henry F., Providence [9].
Walworth, Reuben H., Saratoga, New York [10].
Warder, Dr. J. A., Cincinnati, Ohio [4].
Warner, H. G., Rochester, New York [11].
*Warren, Dr. John C., Boston [1].
Watertown, Charles, Wakefield, England [1].
Wayland, Dr. Francis P., Providence [9].
Wayne, Rev. Benjamin, New Orleans [10].
*Webster, H. B., Esq., Albany [1].
*Webster, Dr. J. W., Cambridge, Massachusetts [1].
*Webster, M. H., Esq., Albany [1].
Webster, Nathan B., Portsmouth, Virginia [7].
Webster, William F., Providence [9].
Weed, Monroe, Esq., Wyoming, New York [6].
Weinland, D. F., Cambridge, Massachusetts [10].
Welch, John, Newark, New Jersey [10].

Wells, David A., New York [2].
Wells, Dr. Thomas, New Haven, Connecticut [4].
West, Charles E., Esq., Buffalo, New York [1].
Wetherell, Prof. L., Legrange, Kentucky [2].
Weyman, G. W., Esq., Pittsburgh, Pennsylvania [6].
Wheatland, Dr. Henry, Salem, Massachusetts [1].
Wheatley, Charles M., New York [1].
Wheeler, T. B., M. D., Montreal [11].
Whipple, A. B., Nantucket, Massachusetts [10].
Whipple, J. E., Lansingburg, New York [10].
Whipple, W., Adrian, Michigan [7].
Whitcomb, Joseph M., Salem, New York [10].
White, Aaron, Cazenovia, New York [10].
White, Charles, Crawfordsville, Indiana [10].
White, Horace, Chicago, Illinois [10].
White, Rev. Robert J. P., Chambly, Canada East [11].
Whitney, Asa, Esq., Philadelphia [1].
Whitney, H. H., Montreal [11].
Whitney, J. D., Esq., Northampton, Massachusetts [1].
Whitney, John R., Philadelphia [11].
Whittlesey, Charles, Cleveland, Ohio [1].
Whittlesey, Charles C., St. Louis, Missouri [11].
Wilder, Alexander, New York [10].
Wilder, L., Esq., Hoosick Falls, New York [1].
Wilgress, George, Montreal [11].
Wilkes, Capt. Charles, U. S. N., Washington, D. C. [1].
Wilkes, Henry, D. D., Montreal [11].
Willard, Samuel D., Cayuga, New York [10].
Williams, Dr. Abraham V., New York [9].
Williams, Henry W., M. D., Boston [11].
Williams, Dr. P. O., Watertown, St. Lawrence Co., N. Y. [6].
Williams, Samuel Wells, Canton, China [10].
Williams, W. F., Mosul, Turkey [10].
Williamson, Prof. James, Kingston, Canada [11].
Wills, Frank, New York [9].
Wilson, Prof. Daniel, Toronto, Canada [10].
Winchell, Prof. Alexander, Ann Arbor, Michigan [3].
Winlock, Prof. Joseph, Cambridge, Massachusetts [5].

Winslow, C. F., Troy, New York [10].
Winslow, John F., Troy, New York [10].
Wood, William, Portland, Maine [10].
Woodall, John W., Scarborough, England [11].
Woodbridge, George A., Nashville, Tennessee [10].
*Woodbury, Hon. L., Portsmouth, New Hampshire [1].
Woodman, John S., Hanover, New Hampshire [11].
Woolworth, Hon. S. B., Albany [10].
Worcester, Dr. Joseph E., Cambridge, Massachusetts [2].
Worthen, A. H., Warsaw, Illinois [5].
Wright, Charles, Wethersfield, Connecticut [10].
Wright, Chauncey, Cambridge, Massachusetts [9].
*Wright, Dr. John, Troy, New York [1].
Wurtele, Louis C., Lennoxville, Canada East [11].
Wurtz, Henry, Trenton, New Jersey [10].
Wyman, Prof. Jeffries, Cambridge, Massachusetts [10].
Wyman, Morrill, M. D., Cambridge, Massachusetts [11].
Wynne, Dr. James, New York [8].
Wynne, Thomas H., Richmond, Virginia [8].

Y.

Youmans, E. L., Esq., Saratoga Springs, New York [6].
Young, Prof. Ira, Hanover, New Hampshire [7].
Young, Hon. John, Montreal [11].

The above list contains ten hundred and fourteen names, of which sixty-eight are of deceased members.

MEMBERS ELECTED

AT

THE MONTREAL MEETING.*

Anderson, Rev. William, Montreal.
*Andrews, Israel W., Marietta, Ohio.
Atterbury, Rev. John G., New Albany, Ind.

*Baby, G., Montreal.
Baldon, A. S., M. D., Jacksonville, Florida.
Bancroft, Rev. Canon, Montreal.
Baudry, J. H., Montreal.
Baylis, James, Montreal.
Beale, James, M. D., Richmond, Va.
Belle, Charles E., Montreal.
Benedict, Thomas B., M. D., Trinity, La.
Benton, Charles, Oxbow, N. Y.
Berezy, William, Daillehaut, C. E.
*Bernard, A., M. D., Montreal.
Bertron, S. R., Port Gibson, Miss.
Bibaud, J. G., M. D., Montreal.
Bidwell, Rev. Walter H., New York.
*Billings, E., Montreal.
Blackmarr, Rev. Henry, Rochester, N. Y.
Blake, Rev. C. M., Chili.
Bonar, Rev. James B., Montreal.
Bradford, James C., M. D., Elyria, Ohio.
Braithwaite, Rev. Joseph, Chambly, C. W.
*Brewer, Fisk P., New Haven.
Briggs, George W., Pittsfield, Mass.
*Bristow, William, Montreal.
Brown, Robert, Jr., Cincinnati.
*Brush, George I., New Haven.
Brush, George S., Montreal.

Cady, Rev. Calvin B., Alburgh, Vt.
*Campbell, A. D., Montreal.
*Campbell, Prof. George W., Montreal.
*Campbell, W. D., Quebec.
Cameron, Hector, Toronto.
*Cartier, Hon. G. E., Montreal.
*Chamberlin, B. C., Montreal.
*Chapin, L. C., New Haven.
*Chapman, Prof. C. B., Madison, Wis.
*Chapman, Prof. E., Jr., Toronto.
Clark, John S., M. D., New Orleans.
Cleghorn, Rev. E. B., New Orleans.
*Cochran, J. W., New York.
*Collier, Prof. G. H., Wheaton, Ill.
*Collins, C., Carlisle, Penn.
*Comstock, J. C., Hartford, Conn.
Conckling, Fred. A., New York.
Copes, Joseph S., M. D., New Orleans.
Cordner, Rev. John, Montreal.
Cowan, J. P., M. D., Montreal.
*Cramp, J. M., Acadia College, N. S.
*Craik, Robert, M. D., Montreal.
*Croft, Prof. Henry, Toronto.
Cummings, William M., New York.
Cunynghame, Thurlow, Montreal.
Curry, Rev. W. F., Geneva, N. Y.
*Curtis, Jasper, St. Albans, Vt.
Cyr, Rev. Narcisse, Montreal.

Dalrymple, Rev. E. A., Baltimore, Md.
*Dalton, Prof. John C., Jr., New York.
Danforth, Edward, Clarence, N. Y.
*David, A. H., M. D., Montreal.

* Those marked with an asterisk paid the assessment, and signed the Constitution without being formally elected.

*Davies, W. H. A., Montreal.
*Davies, W. H., Toronto.
*Day, Charles D., Montreal.
Day, John J., Montreal.
Dexter, G. M., Boston.
Dickson, Andrew, Kingston, Can.
*Disturnell, John, New York.
Doolittle, Rev. L., Lennoxville, C. E.
Dorr, Robert L., Danville, N. Y.
Dowie, J. Muir, Liverpool, Eng.
*Doyle, John P., Montreal.
*Dunkin, Christopher, Montreal.
Dwinelle, John H., Rochester, N. Y.

Eaton, A. H., New York.
*Edmondstone, William, Montreal.
Elin, Frederick, London.

Fairchild, Charles, Madison, Wis.
Fairhaven, James, Montreal.
*Fairly, Dr. David, Glasgow, Scot.
Fair, Samuel, M. D., Columbia, S. C.
Fay, Rev. Charles, St. Albans, Vt.
*Ferland, J. B. A., Quebec.
Ferrel, William, Nashville, Tenn.
*Feuchtwanger, Louis, M. D., N. Y.
Field, Roswell, Gill, Mass.
*Fisher, Prof. George P., New Haven.
*Fitch, Edward H., Ashtabula, Ohio.
Folsom, George, New York.
*Franklin, Danforth, Buffalo, N. Y.
Fraser, Hugh, Montreal.
*Fraser, Prof. W., M. D., Montreal.
French, J. H., Syracuse, N. Y.
*French, Rev. J. W., West Point.
*Fristoe, Edward T., Washington.
Frothingham, George H., Montreal.
Frothingham, Rev. Fredric, Portland, Me.

Gale, Hon. Samuel, Montreal.
*Garth, Charles, Montreal.
Geddes, Charles, Montreal.
Gibb, J. Campbell, Montreal.
Ginder, Henry, New Orleans.
*Gould, A. A., M. D., Boston.
*Grant, John M., Montreal.
*Grant, S. Hastings, New York.
Greene, F. C., M. D., East Hampton, Mass.
Greenshields, J B., Montreal.
*Guerin, Thomas, Albany.

Hager, Albert D., Proctorsville, Vt.
*Harris, Ira, Albany.
Harrison, B. F., Wallingsford, Conn.
*Harrison, Edwin, St. Louis, Miss.
Hart, Theodore, Montreal.
Headlam, William, Jr., Albany.
Henderson, Rev. Isaac J., New Orleans.
Heneker, William R. Sherbrooke, C. E.
Hickok, Rev. M. J., Scranton, Penn.
*Hilgard, Eugene W., Washington.
*Hill, Benjamin, Montreal.
*Hincks, Rev. William, Toronto, C. W.
Hitchcock, Charles H., Amherst, Mass.
*Hodgins, George, Toronto, C. W.
*Holmes, Prof. A. F., Montreal.
*Holmes, Benjamin, Montreal.
*Holton, L. H., Montreal.
*Holwell, W. A., Quebec.
*Homes, Henry A., Albany.
Hopkins, Rev. J. H., New York.
Houghton, George F., St. Albans, Vt.
*Howard, R. P., M. D., Montreal.
*Howe, Prof. Henry A., Montreal.
Hulburd, Hon. Calvin T., Brasher Falls, N. Y.
Humphrey, Frederick, Iowa City, Io.
*Humphreys, Capt. A. A., U. S. A., Washington.
Hunter, Andrew W., New Orleans.
Hunter, George W., New Orleans.
Hunt, Freeman, New York.
Hurlburt, Dr. J., Hamilton, C. W.
Huson, Calvin, Jr., Rochester, N. Y.

Inglis, Rev. David, Hamilton, C. W.

*Jack, Prof. W. B., Fredericton, N. B.
Janes, D. P., Montreal.
*Joseph, J. H., Montreal.

Keefer, Samuel, Brockville, C. E.
*Keefer, Thomas C., Hamilton, C. W.
*Kemp, Rev. Alexander, Montreal.
Kerr, Robert C., New Orleans.
*Kiershowski, A. E., Montreal.
*Kingston, G. T., Toronto.
*Kinnear, David, Montreal.
Kittredge, Dr. Josiah, South Hadley Mass.
Knox, Rev. John, Newtown, L. I.

*Lachlan, Major R., Cincinnati, Ohio.
Laflamme, G., Montreal.
*Laflamme, Prof. Rodolphe, Montreal.
*Latour, L. A. H., Montreal.
*Lawford, Frederick, Montreal.
Leckie, Robert, Montreal.
Lindsay, William B., New Orleans.
Little, Weare C., Albany.
Loranger, Hon. T. W., Montreal.
Louson, John, Montreal.
Lovelace, P. E. H., M. D., New Orleans.
*Lunn, William, Montreal.
*Lyman, Henry, Montreal.
*Lyman, Theodore, Montreal.

Lyon, Merrick, Providence.

*Mac Callum, Prof. D. C., Montreal.
Mackay, J. P., Montreal.
*Mason, Charles, Burlington, Io.
*Mathieson, Alex., D. D., Montreal.
McChesney, J. H., Springfield, Ill.
McClory, Rev. Henry, Warehouse Point, Conn.
*McCord, Hon. J. L., Montreal.
McCormick, Richard C., Jr., New York.
McElroy, John E., Albany.
McIlvaine, Rev. J. H., Rochester, N. Y.
Mackay, Joseph, Montreal.
Meilleur, J. B., M. D., Montreal.
Meredith, Edmund P., Toronto.
Merrill, Stephen, Charlestown, Mass.
Millar, G. M., Montreal.
Mills, Charles C., M. D., Richmond, Va.
*Miles, Prof. Henry H., Lennoxville, C. E.
Moffatt, George, Montreal.
Moffatt, J. O., Montreal.
*Mondelet, Judge Charles, Montreal.
Montgomerie, Hugh E., London.
Morris, J. R., Houston, Texas.
Morrow, E. G., Cambridge, Mass.
Morse, Charles M., Waterville, Me.
*Mowat, O., Toronto.
*Munro, Col. William, Montreal.
*Murphy, Edward, Montreal.
*Murray, Alexander, Woodstock, C. W.
Murray, Prof. David, Albany.
Myers, Gustavus A., Richmond, Va.

*Nason, Rev. Elias, Natick, Mass.
*Nault, Dr. J. Y., Quebec.
*Nelles, Pres. S. S., Victoria College.
*Newell, W. A., Trenton, N. J.
*North, Prof. Edward, Clinton, N. Y.

Paradis, H. C. A., M. D., Montreal.
Parker, Rev. Henry E., Concord, N. H.
*Paton, George, Galt, C. W.
Paton, John, Toronto.
*Patterson, Prof. J. W., Hanover, N. H.
Perkins, Louis, New London, Conn.
*Perkins, J. A., Montreal.
Phelps, Prof. W. F., Trenton, N. J.
Powell, Robert I., Illinois.
*Pynchon, Prof. Thomas R., Hartford, Conn.

Quincy, Edmund, Jr., Dedham.

*Ramsay, Andrew C., London.
Ramsay, J. K., Montreal.
Rauch, John H., M. D., Burlington, Io.
*Read, D. B., Toronto.
Redfield, Charles B., Albany.
Richards, Newton, New Orleans.
Robertson, Rev. W. H. C., Stamford, Conn.
*Robinson, W. B., Toronto.
Rockwell, John, Chicago, Ill.
Rogers, Prof. Fairman, Philadelphia.
*Roosevelt, Clinton, New York.
*Rose, Henry, Montreal.
Roy, Euclide, Montreal.
Russell, Andrew, Toronto.
Russell, Archibald, New York.
Ruttan, Allan, M. D., Newburgh, C. W.
*Ryan, Thomas, Montreal.
Ryerson, Rev. E., Montreal.

Sanderson, Rev. J. E., Montreal.
Scott, Rev. Joseph, Durham, C. E.
*Scott, Rev. William, Peterborough, C. W.
Scripps, John L., Chicago.
*Seemann, Berthold, Ph. D., London.
Seymour, M. H., Montreal.
Shelton, E. E., Montreal.
*Sheppard, William, Drummondville, Can.
*Sherwin, Thomas, Dedham, Mass.
Shotwell, Samuel L., Macedon, N. Y.
Skipwith, P. H., New Orleans.
Smith, Gen. John Spear, Baltimore.
Smith, John, Montreal.
*Smith, Spencer, St. Louis, Missouri.
Snodgrass, Rev. William, Montreal.
*Sola, Rev. Dr. A., Montreal.
Spink, William, Toronto.
Sprague, Daniel J., South Orange, N. J.
Sprague, Rev. Daniel G., South Orange, N. J.
*Streeter, S. F., Baltimore.
Stuart, Prof. A. P. S., Acadia, N. S.
Swann, Hon. Thomas, Baltimore.

*Taylor, J. W., Montreal.
*Taylor, Rev. William, Montreal.
*Taylor, Thomas M., Montreal.
Thickstun, I. F., Meadville, Pa.
*Thomas, Richard, Montreal.
Thomson, Rev. John, New York.
Tolderoy, James B., M. D., Fredericton, N. B.
*Treadwell, C. P., L'Original, C. W.
*Trudeau, Alexis, Montreal.
Tyler, Robert S., Montreal.

Ulffers, A. H., Springfield, Ill.
Ulrici, Richard W., St. Louis.

Van Cortlandt, Dr., Ottawa, C. W.

Wadsworth, Charles F., Geneseo, N. Y.
*Wallbridge, I. C., Toronto.
Warner, H. G., Rochester, N. Y.
Watertown, Charles, Wakefield, Eng.
Wetherill, Samuel, Bethlehem, Pa.
*Wheeler, T. B., M. D., Montreal.
White, Rev. Robert J. P., Chambly, C. E.
Whitney, H. H., Montreal.
Whitney, H. N., Montreal.
Whitney, John R., Philadelphia.
*Whittlesey, Charles C., St. Louis, Mo.
*Wilgress, George, Montreal.
*Wilkes, Henry, D. D., Montreal.
Williams, Henry W., M. D., Boston.
*Williamson, Prof. James, Kingston, Can.
*Woodall, John W., Scarborough, Eng.
*Woodman, John S., Hanover, N. H.
Woodruff, H. W. B., New York.
Wright, Joseph C., Oswego, N. Y.
Wurtele, Lewis C., Lennoxville, C. E.
*Wyman, Morrill, M. D., Cambridge, Mass.

*Young, Hon. John, Montreal.

The following paid the assessment, but were not formally elected, and did not sign the Constitution.

Beaubien, Dr.
Brydges, Sir Harford J.
Brinsmade, Dr. T. C.

Court, James.

Davis, W. H.
Dougall, J.

Fenwick, George E., M. D.

Gibb, J. C.

Hilton, W.

Jackson, Stephen.

Lench, Dr.

M'Gill, Hon. Peter.
Mackay, R.

Peel, T. B.
Perrault, O.

Walden, Dr. A. S.
Weaver, G. W.
Wykoffe, J. L.

The following signed the Constitution, but were not elected, and did not pay the assessment.

Abbott, J. H.

Bacon, J. R., Bahama.
Bancroft, Charles, St. Johns, C. E.

Dairs, William M., Cincinnati.
Douglas, Dr. George M., Quebec.
Douglas, James, Quebec.
D'Urban, William S. M., Montreal.

Harley, Lieut., 3d W. I. Regiment.

Lyman, S. J.

Poe, David A., Montreal.

Redpath, Peter, Montreal.
Robertson, Duncan, Montreal.
Rodden, William, Montreal.

Sandborn, Edwin D., Hanover, N. H.

Tompson, J. H., Lennoxville, C. E.

Whitney, N. S., Montreal.

The following have voluntarily withdrawn from the Association, or declined their election.

J. B. Angell.
J. H. Alexander.
William Goddard.
Roswell Field.
Owen Mason.
Dr. T. R. Ingalls.
Josiah W. Gibbs.

AN ADDRESS

IN COMMEMORATION OF

PROFESSOR J. W. BAILEY,

PRESIDENT OF THE ASSOCIATION,

DELIVERED BEFORE THE ASSOCIATION, AUGUST 19, 1857,

BY DR. A. A. GOULD.

MR. PRESIDENT, AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, —

We are called upon, at this time, to advert to an event such as has not before occurred in the history of this Association during the seventeen years since its inception. He, who was elected at our last session to preside at this meeting, has in the mean time been taken from us. He, whom we all delighted to honor, though he sought not public honors from men, and who required no higher stimulus to his ambition nor reward of his toil than the satisfaction derived from the discovery and elucidation of the works and laws of Nature's God, has left this arena of trial and doubt to meet that God; and in His presence, where faith is turned to sight, we may well believe that he is unfolding with delight, unmingled with doubt, those wonderful works and perfectly harmonious laws which so engaged and delighted him on earth.

It is becoming that we should bestow a few moments in commemoration of his life and labors. There are others, who knew him much better, and were more conversant with his special studies than myself, to whom this office properly belonged; but as in another connection I had gathered some of the items of his history, and had noted the results of his scientific investigations, I have been requested to present them on this occasion.

JACOB WHITMAN BAILEY was born April 29, 1811, in the old town of Ward, Mass. (now Auburn), at the residence of his grandfather, Rev. Isaac Bailey, the first minister of that town. In infancy he was removed to Providence, and there received his early education in the ordinary schools of that day. The limited resources of his family rendered it necessary that he should early engage in some employment; and, at the age of twelve years he was placed at a Circulating Library, where he attracted the attention of visitors by his studious devotion to books. At this time also he began a collection of shells and insects. During a visit of the West Point cadets at Providence, he became acquainted with some of the officers, and then decided to seek admittance to the Military Academy. He received an appointment as cadet in July, 1828, and graduated July, 1832. He was appointed second lieutenant in the artillery, April, 1833, and was promoted to first lieutenant, February, 1837. During this time he was stationed at Old Point Comfort, Bellona Arsenal, and Fort Moultrie. But with the development of his scientific tastes, military life had few attractions for him; and in 1839 he received the more congenial appointment of Professor of Chemistry, Mineralogy, and Geology in the United States Military Academy, which appointment he held, first as assistant, and soon afterwards as principal, until his death. He was married in 1835, and, with his wife and only daughter, then seventeen years of age, was on board the steamer Henry Clay, which took fire on the Hudson river, July 28, 1852. After having lowered them successfully to the water, and received from them the assurance of their safety, he proceeded to follow, when suddenly a volume of smoke and flame veiled them from his view, and they were lost. He had previously been subject to a bronchial affection and occasional spitting of blood, for which he had resorted to Florida the previous winter, with decided benefit. But the exertion and exposure on this occasion, together with the intensity of his bereavement, gave him a shock, from which he never rallied. With the exception of an occasional resort to the sea-shore during vacations, he was afterwards obliged to exclude himself almost entirely from society. His health steadily declined; and, feeling the certainty of the issue, he employed his leisure in arranging his papers, his microscopical collections, and his Algæ, so that they might be practically available to his successors. He died on the 27th of February last, at the age of forty-six.

As a man, he was remarkably unobtrusive and modest, gentle in his manners, truthful in his character, cordially beloved by all who had the good fortune to enjoy his acquaintance.

But it is more with his scientific position that we are concerned. His taste for science was very early developed. Beginning with botany and mineralogy, and passing from those to geology, chemistry, and microscopy, he traversed a large portion of the field of natural science. In the departments more especially relating to his position at West Point he held a high rank, and his publications show that he introduced many improvements in chemical manipulation. His correspondence, too, shows that he was extensively consulted by men of science on some of the most difficult points of analysis and general physics. His observations were always of the most careful and accurate character; and he early began the important practice of making notes of them, accompanied by delineations, leaving nothing to recollection or mere indefinite statements; thus having always at hand, permanent data for his subsequent papers. The volume containing these, which he denominated "Microscopic Sketches," is, of itself, a surprising evidence of his industry and skill. There are four hundred and fifty sheets, containing about three thousand sketches. By his great skill with the pencil he rendered himself independent of artists, an accomplishment, for the lack of which many of the best observers lose their labors. These drawings date back as far as 1838, twenty years ago, and enable us to trace out the course of his studies, as well as his wanderings; for wherever he went, his microscope or his collecting bottles went with him. At first, we have mostly sketches of vegetable and animal tissues, and occasionally an entire animal or plant. In January, 1839, while examining some aquatic plants, he perceived a curious object, a Gomphonema as it subsequently proved, which he did not understand. This excited his attention in that direction, and soon we find many others of the more common Diatoms delineated. In March, 1839, he sketched a new one, to which Ehrenberg gave the complimentary name *Stauronema Baileyi*; and finally he devoted himself with great zeal to the varied objects included under the general term Infusoria, and also to a department almost equally demanding his skill as a microscopist, namely, the Algæ. So far as the Infusoria were concerned, he stated, in 1843, that no one else in this country had studied them; and that it was almost impossible to procure any works relating to them. Ehrenberg's work he had not seen, though he modestly utters the thought that Ehrenberg might sometime see and correct his paper. He, however, gradually possessed himself of all the important works on those subjects, and became the active correspondent of Ehrenberg, Kützing, Agardh, Queckett, Ralfs, Harvey, Greville, DeBrébisson, Montagne, and very many others. Fossil deposits, mud, and guano, were collected from every quarter for investigation. The various exploring expeditions were laid under contribution; and more recently, the objects brought up on the sounding lead in the coast survey, and by Lieut. Berryman's line of soundings across the Atlantic, made in reference to the laying of the telegraphic cable, occupied his attention. In pursuing these examinations he found the relics from the bottom so well characterized in certain localities and at certain depths, that he suggested the possibility of being able, in some instances at least, to determine the safety or otherwise of a vessel, by an examination of the organisms brought up on the sounding lead, when prevented by darkness, snows, or fogs, from deciding by ordinary observations.

Not a little of the obligations of microscopists to Prof. Bailey is due for his labors to improve the microscope. At any rate, few among us have ventured upon the purchase of a valuable instrument, without first consulting him in reference to it, and perhaps taxing him with unwelcome negotiations; and his letters show that numerous applications of this kind must have been a most serious tax upon his time. It is said that his own early observations were made with globules of glass blown by himself. After he became possessed of a proper instrument, many modifications in the construction of the stage and its movements, and in other appendages, were made by him; and it is to his experience and scientific deductions, coupled with the genius and incomparable mechanical skill of Spencer, that we are indebted for the most powerful microscopes that have yet been made. His masterly and triumphant defence of them against the detractions of transatlantic pens, also exhibits his complete mastery of the subject. One of his last essays was to construct an Indicator, by means of which the place of any object on a slide might readily and certainly be found. No one, in looking at the card, would credit the labor and thought which he, in conjunction with his friends, Judge Johnson and Mr. Gavitt, bestowed upon it. Many futile efforts were made, and many quires were used in correspondence, before the accuracy of its measurements, and a method for the unerring application of it, were satisfactorily accomplished.

At a very early date, Prof. Bailey began to publish the results of his observations, — a duty too often neglected by scientific men. His published papers are very numerous, — more than fifty, — extending as far back as 1837, and up to his very last hours. They were, for the most part, very brief, free from ostentation, aiming to communicate facts in the simplest and most direct manner. In the words of his friend, Prof. Gray, "they are all clear, explicit, and unpretending as they are thorough; and every one of them embodies some direct and positive contribution to science." Most of them were terminated by a condensed statement of the general facts elicited, so as to show, at a glance, the subject, and result arrived at. They are mostly to be found in Silliman's Journal of Science, or in the Smithsonian Contributions to Knowledge, except one in the first volume of the Transactions of the Association of Geologists and Naturalists, which embodied his previous papers on the Infusoria of the United States, with additions, and which gave him at once a high position as a scientific naturalist.

His MICROSCOPICAL COLLECTION will constitute his most splendid monument. The slides, of which there are five hundred and fifty, are arranged in boxes in the form of octavos, of which there are twenty-four volumes. More than three thousand objects, fixed upon slides, are catalogued and noted with reference to Bailey's Indicator, thus enabling any one readily to find with certainty the identical specimens described by him. There are also very many other slides not included in the regular collection. Being objects either described by himself or given to him by other describers, this collection must always possess the highest authority, and must be our ultimate reference in all cases of doubt.

The COLLECTION OF ALGÆ is equally complete and authentic. It consists of thirty-two portfolios, containing about 4,500 specimens; and it may safely be said that few collections in the world are superior to it.

It is probably well known that Prof. Bailey bequeathed his

Microscopical Collection, his Collection of Algæ, his books on Botany and Microscopy, his Memoranda and his Scientific Correspondence, to the Boston Society of Natural History. While the Society intends to keep this bequest sacredly, it means also to make it as extensively useful as possible. I hesitate not, in behalf of the Society, to invite all who are pursuing similar researches to consult these collections, whenever convenient, — and to give assurance also, that any questions which may be solved by it may be freely addressed to the Society. A large collection of rough material for microscopic research, from many of the most interesting localities, is also in the possession of the Society, and will be distributed to microscopists and societies.

Such are some of the principal events in the history of our distinguished associate and President, and such are some of the accumulated fruits of his scientific labors, — labors which were performed in addition to the full duties of a professorship, executed with military precision and punctuality. He may well be styled the Ehrenberg of America, and has won for himself a place by the side of the most eminent microscopists and algologists of the old world. He will always stand as the father, in this country, of those branches of Natural History which relate to the *world of atoms*, and must for ever remain the standard reference here in relation to them. Let no man think lightly of them because they relate to little things, too small to be discerned by the unassisted eye. Are they not equally the handiwork of Him who made and sped the spheres, and formed man in his own image? And if he, by the microscope, demonstrated the vegetable structure of coal, illustrated the lowest habitable depths of the ocean, settled the nature of some of the important geological strata, and of the vast deserts otherwise deficient in geological indications, — questions of practical importance in our investigations of the crust of the earth, let him receive a corresponding rank with him who points the telescope to the mighty orbs above, determines their magnitudes and movements by scientific induction, and thereby enables us to determine our place upon that crust.

I cannot refrain from quoting, in conclusion, the words of an intimate friend in a letter to him, on learning of his appointment as President for this meeting. He says, "I am sure every one acquainted with what you have done for the advancement of science, American science, and American scientific character, will say, that no appointment, at the present time, could be more appropriate or just. I hope the great Disposer of events, whose minute works you have done so much to place before our eyes in all their exquisite beauty of form, of workmanship, and of adaptation, will give you yet many years to enjoy the honors you have so honestly acquired, and to add many more discoveries to those you have already secured." And may I not respond for you all, Would that this desire had been granted!

AN ADDRESS

IN COMMEMORATION OF

WILLIAM C. REDFIELD,

FIRST PRESIDENT OF THE ASSOCIATION.

DELIVERED BEFORE THE ASSOCIATION, AUGUST 14, 1857,

BY PROFESSOR DENISON OLMSTED, LL.D.

MR. PRESIDENT, AND GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE, — Since last we met, the Destroyer has been very busy in our ranks. Besides other beloved and respected associates, our earliest and our latest Presidents have suddenly vanished from our midst; — Redfield, who was the first to suggest the idea of the American Association on its present comprehensive plan, and the first to preside over its deliberations, and Bailey, who, we fondly hoped, would occupy the same distinguished position on the present occasion. From the vision of both, as we humbly trust, the veil which permits us here to see only through a glass darkly is removed; and the grand laws of Nature, and the infinitesimal, no less than the infinite in God's works, are revealed to them in the clear light of heaven.

With Mr. Redfield my acquaintance has been long and intimate. I was conversant with his earliest researches on the subject which is so closely associated with his name, and I have been constantly a witness of his untiring, self-sacrificing labors in the cause of science, through all the subsequent years of his life. I respected him as a man; I admired him as a philosopher; I loved him as a friend. We miss him here, always the earliest to come and the latest to depart. We miss his gentle tones, his kindly greetings. We miss still more the radiance which his clear mind cast upon our pathway up the hill of science. I am thankful for the opportunity of presenting before this learned assembly a synopsis of his scientific labors. Some brief notice also of his personal history may be acceptable, not only to satisfy the wishes of his friends, but for the benefit of his example, which, I trust, will especially commend itself to the self-taught votary of science, and to all who are engaged in the pursuit of knowledge under difficulties, both as an incentive and a model. A life passed in the ordinary walks of business or in the quiet of philosophical research, affords little of that romantic incident which lends a charm to biography; still we think the life of Mr. Redfield affords an interesting and instructive theme for contemplation in a threefold point of view,—as affording a marked example of the successful pursuit of knowledge under difficulties,—as happily illustrating the union in the same individual, of the man of science with the man of business,—and as exhibiting a philosopher, whose researches have extended the boundaries of knowledge, and greatly augmented the sum of human happiness.

William C. Redfield was born at Middletown, Connecticut, on the 26th* of March, 1789. He was of pure English descent, both by the father's and mother's side. His father, from a natural love of adventure, chose in early youth a seafaring life, and afterwards followed the seas as a profession to

* Printed erroneously in Silliman's Journal, "25th of March."

the time of his death, which happened when this, his eldest son, was only thirteen years old. His early training, therefore, devolved chiefly on his mother, who was a woman of superior mental endowments and of exalted Christian character.

The slender pecuniary resources of the family would not allow young Redfield any opportunities of school education beyond those of the common schools of Connecticut, which, at that time, taught little more than the simplest rudiments — reading, spelling, writing, and a little arithmetic; and all access to the richer treasures of knowledge seemed to be forever denied him, when, at the early age of fourteen, he was removed to Upper Middletown, now called Cromwell, and apprenticed to a mechanic, whose tasks engrossed every moment of his time except a part of his evenings. These brief opportunities, however, he most diligently spent in the acquisition of knowledge, eagerly devouring every scientific work within his reach. He was denied even a lamp for reading by night much of the time during his apprenticeship, and could command no better light than that of a common wood fire in the chimney corner. Under all these disadvantages, it is evident that, before he was twenty-one years of age, he had acquired no ordinary amount and variety of useful knowledge. During the latter part of his apprenticeship, he united with other young men of the village in forming a debating society, under the name of the "Friendly Association," with which was connected a small but growing library. To this humble literary club Mr. Redfield always ascribed no small agency in inspiring him with a love of knowledge, and a high appreciation of its advantages; and during his future years, he nursed and liberally aided by his contributions this benefactor of his youth.

Fortunately for young Redfield, a distinguished and learned physician, Dr. William Tully, fixed his residence in the same village, and generously opened to him his extensive and well-selected library; and, what must have been equally inspiring to youthful genius, Dr. Tully furnished him with a model of an enthusiastic devotee to knowledge, and of a mind richly stored with intellectual wealth. The modest youth who first presented himself as a suppliant for the loan of a book from the Doctor's library was soon recognized as a congenial spirit, and was admitted to an intimate friendship, which lasted to the day of his death. Dr. Tully has favored us with the particulars of his first acquaintance with our friend. On his application for a book to occupy such moments as he could redeem from his daily tasks, the Doctor, being then ignorant of his acquirements or his taste, opened different cases of his library, submitting the contents of each to his selection. Among a great variety of authors, that which determined his choice was Sir Humphrey Davy's Elements of Chemistry. As this was one of the earliest systematic works that contained the doctrine of Chemical Equivalents, a subject then considered as peculiarly difficult, and one understood by few readers of the work, the Doctor had little expectation that this young inquirer after knowledge would either understand or relish it. In a short time he returned the book, and surprised the Doctor by evincing a thorough acquaintance with its contents, and expressing a high satisfaction, in particular with the doctrine of chemical equivalents, which, he said, he had then met with for the first time.

Some time before young Redfield reached the end of his apprenticeship, his widowed mother had married, and removed to the State of Ohio. He was no sooner master of his time than he set out on foot to pay her a visit in her new home, distant more than seven hundred miles. It was a formidable undertaking, at that early period before the age of steamboats and railways, and when a large part of the way was covered with dense forests, with hardly an open path even for the pedestrian. Stage-coaches, indeed, ran on the nearer portions of the route, but these were too expensive for the slender finances of our young adventurer. Accompanied, therefore, by two other young men, he shouldered his knapsack, and commenced the arduous journey. Every evening he noted down the incidents and observations of the day. This journal is now in my possession, and I have perused it with deep interest for the graphic sketches it contains of the countries he passed through, then mostly new settlements, and for the indications it affords of those powers of observation which afterwards led to the development of the laws of storms. The style of composition is far superior to what might reasonably have been expected from one who had enjoyed so few literary advantages, evincing two qualities for which Mr. Redfield was always distinguished—good sense and good taste. The sketches of Western New York, and of Northern Ohio, taken while the sites of Rochester and Cleveland were dark and gloomy forests, and Buffalo was a mere hamlet, possess no ordinary degree of historical interest. Instead of a "Lake Shore" road, traversed by the iron horse, as at present, our young pedestrians could find no better paths in which to travel over the southern side of Lake Erie than to course along the beach. Yet in twenty-seven days they made good their journey, having rested four days on the way, making an average of about thirty-two miles per day. After passing the winter with his friends in Ohio, he resumed his way homeward, on foot and alone, returning by a more southern route, through parts of the States of Virginia, Maryland, and Pennsylvania. We shall soon see to what valuable account he afterwards turned the observations made on these early pedestrian tours, in tracing the course, as well as originating the project, of a great railway connecting the Hudson and the Mississippi rivers.

Returning to his former home in 1811, Mr Redfield commenced the regular business of life. No circumstances could seem more unpropitious to his eminence as a philosopher, than those in which he was placed for nearly twenty years after his first settlement in business. A small mechanic in a country village, eking out a scanty income by uniting with the products of his trade the sale of a small assortment of merchandise,

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Mr. Redfield met with obstacles which, in ordinary minds, would have quenched the desire of intellectual progress. Yet every year added largely to his scientific acquisitions, and developed more fully his intellectual and moral energies. Meanwhile his active mind left its impress on the quiet community where he lived, in devising and carrying out various plans for advancing their social comfort and respectability, in the improvement and embellishment of their streets, school-houses, and churches, and in promoting the interests of the literary club, from which he himself, in early youth, had derived such signal advantages. From deep domestic trials which afflicted him about the year 1820, he had recourse for solace both to the word and the works of God. It was soon after one of the severest of these trials that his attention was first directed to the subject of Atlantic Gales.

On the 3d of September, 1821, there occurred, in the eastern part of Connecticut, one of the most violent storms ever known there, and long remembered as the "great September gale." Shortly after this, Mr. Redfield, being on a journey to the western part of Massachusetts, happened to travel over a region covered by marks of the ravages of the recent storm. He was accompanied by his eldest son, then a young lad, who well remembers these early observations of his father, and the inferences he drew from them. At Middletown, the place of Mr. Redfield's residence, the gale commenced from the south-east, prostrating the trees towards the north-west; but on reaching the north-western part of Connecticut, and the neighboring parts of Massachusetts, he was surprised to find that there the trees lay with their heads in the opposite direction, or towards the south-east. He was still more surprised to find, that, at the very time when the wind was blowing with such violence from the *south-east* at Middletown, a *north-west* wind was blowing with equal violence at a point less than seventy miles distant from that place. On tracing further the course and direction of prostrated objects, and comparing the times when the storm reached different places, the idea flashed upon his mind that the storm was a *progressive whirlwind.* A conviction thus forced upon his mind, after a full survey of the facts, was not likely to lose its grasp. Amid all his cares, it clung to him, and was cherished with the enthusiasm usual to the student of nature, who is conscious of having become the honored medium of a new revelation of her mysteries. Nothing, however, could have been further from his mind than the thought, that the full development of that idea would one day place him among the distinguished philosophers of his time. So little, indeed, did he dream of fame, that, for eight or nine years after the first conception of his theory, he gave little attention to the study of the phenomena of storms, but was deeply engrossed in other enterprises, which, although foreign to this subject, were alike evincive of his original and inventive turn of mind. Of these we may take a passing notice.

Before the scientific world, Mr. Redfield has appeared so exclusively in the character of a philosopher, especially of a meteorologist, that they have been hardly aware of the important services he has rendered the public in the character of *naval engineer*, particularly in the department of steamboat navigation. His attention was turned professionally towards this subject as early as the year 1820, when he became much interested in an experiment with a small boat propelled by an engine of new and peculiar construction, the invention of Franklin Kelsey, Esq., a townsman of his. Although the enterprise was not successful to the company, yet to himself it was not destitute of valuable results, as it was the occasion of his acquiring a more intimate knowledge of the properties of steam, of steam navigation, and of ship-building. On the ruins of that enterprise was erected another, which, after some vicissitudes, acquired a permanent success, and opened to him a sphere of professional labor, which constituted ever afterwards the leading object of his life, as a man of business. Several disastrous steamboat explosions had spread alarm through the community, and created a general terror of steamboats. Redfield was the first to devise and carry into execution the plan of a line of *safety barges* to ply on the Hudson between New York and Albany. The scheme was to construct a passenger boat to be towed by a steamboat at such a distance from it as to avoid all apprehension of danger to the passengers. Large and commodious barges were built, fitted up with greater taste and luxury than had at that time been exhibited by steamboats. With these were connected two large and substantial steamers; and in the excited state of the public mind, these safety barges became great favorites with travellers, especially with parties of pleasure. But our countrymen never hold their fears long; a short interval of exemption from steamboat accidents ended the excitement, while the greater speed attained by the ordinary boats, and the lower fare, gradually drew off passengers from the safety barges, until they could be no longer run with profit to the company, and were abandoned. But the idea was not without profit, for it suggested to him the system of *tow-boats* for conveying freight, which was established in the spring of 1826, and still continues under its original organization. The fleets of barges and canal boats, sometimes numbering forty or fifty, which make so conspicuous a figure on the Hudson river, were thus set in movement by Mr. Redfield, and for thirty years the superintendence of the line first established constituted the appropriate business of our friend. In its management he employed unwearied industry, superior mechanical genius for contriving expedients, and a knowledge of both the science and art of steam navigation, possessed by few men of business. Seldom have we seen the inductive philosopher so happily united with the practical engineer, each character borrowing aid from the other. I know not that any other man connected with the management of a steam-navigation concern, as his profession, ever carried into his business more of the spirit of true science; and it is chiefly on this account that I have thought it fitting to attend our associate into the familiar walks of business, for the purpose of seeing how compatible, and how productive of useful results, is the happy union, in the same person, of the philosopher and the man of business. No one else would have so thoroughly collected the statistics of the profession in this country, embracing all the facts relating to the explosion of steamboat boilers, as they successively occurred, the number of lives lost, the number of deaths by steam compared with those by lightning, and the number compared with those lost by other modes of travel. Moreover, while Mr. Redfield was diligently pursuing his daily business, and conducting with success the affairs of the "Steam Navigation Company," he was also collecting facts for the improvement of the art itself, or for securing the safety of passengers. He divised simpler, cheaper, and safer forms of apparatus than those in general use. He investigated the influence of legal enactments for regulating steam navigation, and pointed out to legislatures and governments the inefficacy or inexpediency of such enactments, and suggested the true measures to be taken to promote the convenience and secure the safety of the public. He addressed a series of letters through the public prints to one of our prominent naval commanders, setting forth the adaptedness of steam as an agent of national defence. He responded to the call of the Secretary of the United States Treasury to point out the causes of steamboat explosions, and to suggest the means of safety. Happy would it be, if in all the great operations of the mechanical arts, the true spirit of the philosopher were so fully conjoined with the practical knowledge and skill of the engineer. How rapid would be the improvement of the arts! How science and art would walk hand in hand, and mutually aid and illustrate each other!

We turn now to another subject which engaged the attention of Mr. Redfield, and brought into exercise his remarkable sagacity and forecast. He was the first to place before the

American people the plan of a system of railroads connecting the waters of the Hudson with those of the Mississippi. His pamphlet containing this project, issued in 1829, is a proud monument of his enlarged views, of his accurate knowledge of the topography of the vast country lying between these great rivers, of his extraordinary forecast, anticipating, as he did, the rapid settlement of the Western States, the magic development of their agricultural and mineral wealth, and the consequent rapid growth of our great commercial metropolis. The route proposed is substantially that of the New York and Erie Railroad, as far as this goes; but his views extended still further, and he marked out, with prophetic accuracy, the course of the railroads which would connect with the Atlantic States the then infant states of Michigan, Indiana, and Illinois. These, he foresaw, would advance with incredible rapidity the settlement of those regions of unbounded fertility, and would divert no small portion of the trade from the Mississippi to the great metropolis of the east.

It must be borne in mind that railroads, for general transportation, were unknown in this country until 1826, when the project of constructing the Albany and Schenectady Railroad was first entertained. As yet the advantages of railroads had not, with us, been practically demonstrated, and especially their advantages over canals were not generally understood or appreciated. At the moment when the Erie Canal, having just been completed, was at the summit of its popularity, Mr. Redfield set forth in his pamphlet, under nineteen distinct heads, the great superiority of railroads to canals, advantages which, although then contemplated only in theory, have been fully established by subsequent experience. He had even anticipated that after the construction of the proposed great trunk railway connecting the Hudson and the Mississippi, many lateral railways and canals would be built, which would bind in one vast network the whole great West to the Atlantic States. "This great plateau (says he) will indeed one day be intersected by thousands of miles of railroad communications; and so rapid will be the increase of its population and resources, that many persons now living will probably see most or all of this accomplished." How well has this remarkable prediction, uttered in 1829, when there was not a foot of railroad in all the country under review, been fulfilled, and how truly has it happened that many of the elder members of this association still live to witness its accomplishment!

The motives which impelled Mr. Redfield to spread this subject before the American people at that early day, when railroads were scarcely known in this country, were purely patriotic. He had no private interests to subserve in the proposed enterprise, and the whole expense of preparing and publishing two editions of the pamphlet embodying these enlarged and prophetic views was defrayed from his own limited resources.

In 1832, Mr. Redfield, in company with Mr. Morgan, civil engineer, reconnoitred the series of interior valleys through which the Harlem Railroad now runs, with a view to the establishment of the New York and Albany Railroad. He was instrumental in obtaining the charter of that road, and published a pamphlet entitled "Facts and Suggestions relating to the New York and Albany Railroad." About the same period, in connection with James Brewster, Esq., of New Haven, he explored the route of a railroad leading from New Haven to Hartford, which afterwards resulted in the construction of the Hartford and New Haven Railroad. As early as 1829, he addressed a memorial to the Common Council of the city of New York, asking permission to lay an experimental railroad in Canal street. The project of a railroad through one of the public streets of New York was, at that time, considered as chimerical, but time has developed the wisdom of the plan, and illustrated the sagacity and forecast that first devised it. . .

When the project of the Hudson River Railroad was started, he entered into it with his characteristic enthusiasm, and was a member of the board of directors which brought that road to its final completion. In the progress of the work he was deeply interested, frequently visiting all parts of the line, and at different periods examining on foot the entire road between New York and Albany. His associates of the board acknowledged themselves indebted to him for many valuable suggestions relating to its construction.*

But we turn from these noble enterprises, in which the philosopher and the engineer were happily united in the same individual, to the consideration of the great subject which, from this time, formed the leading object of his life, namely, to perfect his *theory of storms.* Nor do we turn away from great practical subjects to such as are merely speculative. The lives and property which Redfield's disinterested labors in behalf of steam navigation contributed to save, would, we believe, be of small amount compared with the sailors and ships which the rules founded on his theory of storms, when fully applied to practice, will save from shipwreck.

We have already seen that the attention of Mr. Redfield was first drawn to the subject of storms in the year 1821, by examining the position of trees prostrated by the great September gale, which passed over Connecticut and the western part of Massachusetts that year. Although he had never lost sight of the theory of storms, yet the multifarious business concerns which engrossed the greater part of his time for a number of years afterwards, prevented his bringing it distinctly before the public until the year 1831. I chanced, at that period, to meet him for the first time on board a steamboat on the way from New York to New Haven. A stranger

* From the outset Mr. Redfield maintained that the low rate of fares at first adopted would prove inadequate to sustain the road, and published in the papers of that day a series of articles to show that the road could not be supported at a less rate than two cents per mile. These views met with much opposition at the time, not only from residents on the line of the road, but from members of the board of directors. But the result has proved the soundness of his judgment on that point.

accosted me, and modestly asked leave to make a few inquiries respecting some observations I had recently published in the American Journal of Science on the subject of hailstorms. I was soon sensible that the humble inquirer was himself a proficient in meteorology. In the course of the conversation, he incidentally brought out his theory of the laws of our Atlantic gales, at the same time stating the leading facts on which his conclusions were founded. This doctrine was quite new to me; but it impressed me so favorably that I urged him to communicate it to the world through the medium of the American Journal of Science. He manifested much diffidence at appearing as an author before the scientific world, professing only to be a practical man little versed in scientific discussions, and unaccustomed to write for the press. At length, however, he said he would commit his thoughts to paper, and send them to me, on condition that I would revise the manuscript and superintend the press. Accordingly, I soon received the first of a long series of articles on the laws of storms, and hastened to procure its insertion in the Journal of Science. Some few of the statements made in this earliest development of his theory he afterwards found reasons for modifying; but the great features of that theory appear there in bold relief. Three years afterwards he published, in the 25th volume of the same journal, an elaborate article on the hurricanes of the West Indies, in the course of which he gives a full synopsis of the leading points of his doctrine, as matured by a more extended analysis of the phenomena of storms than he had made when he published his first essay.

Possibly some of those whom I have the pleasure to address may not have fully acquainted themselves with Redfield's theory of storms, and would desire to be informed of its leading principles. I understand this theory to be, substantially, as follows:—

That all violent gales or hurricanes are great *whirlwinds*, in which the wind blows in circuits around an axis either vertical or inclined; that the winds do not move in horizontal circles, as the usual form of his diagrams would seem to indicate, but rather in spirals towards the axis, a descending spiral movement externally, and ascending internally.

That the *direction of revolution* is always uniform, being from right to left, or against the sun, on the north side of the equator, and from left to right, or with the sun, on the south side.

That the *velocity of rotation* increases from the margin towards the centre of the storm.

That the whole body of air subjected to this spiral rotation is, at the same time, *moving forward* in a path, at a variable rate, but always with a velocity much less than its velocity of rotation, being at the minimum, hitherto observed, as low as four miles, and at the maximum forty-three miles, but more commonly about thirty miles per hour, while the motion of rotation may be not less than from one hundred to three hundred miles per hour.

That in storms of a particular region, as the gales of the Atlantic, or the typhoons of the China seas, *great uniformity exists in regard to the path pursued*, those of the Atlantic, for example, usually issuing from the equatorial regions eastward of the West India islands, pursuing, at first, a course towards the north-west, as far as the latitude of 30°, and then gradually wheeling to the north-east, and following a path nearly parallel to the American coast, to the east of Newfoundland, until they are lost in mid-ocean; the entire path when delineated, resembling a parabolic curve, whose apex is near the latitude of 30°.

That their *dimensions* are sometimes very great, being not less than one thousand miles in diameter, while their path over the ocean can sometimes be traced for three thousand miles.

That the *barometer*, at any given place, falls with increasing rapidity as the centre of the whirlwind approaches, but rises at a corresponding rate after the centre has passed by; and finally,

That the phenomena are more uniform in large than in small storms, and more uniform on the ocean than on the land.

These laws Mr. Redfield claims as so many *facts* independently of all hypothesis; as facts deduced from the most rigorous induction, which will ever hold true, whatever views may be entertained respecting the origin or causes of storms.

The *method* adopted by the author of this theory, in all his inquiries, — the method which first led him to the discovery of the whirlwind character of storms, and afterwards fully confirmed the doctrine, — was first to collect and then to collate as many records as possible of vessels that had been caught in the storm, in various parts of the ocean. The most laborious and profound investigation of this nature of which he has left us an example is in the case of the Cuba hurricane of October, 1844. First, he examined all accessible marine reports of vessels that had arrived in port after encountering the storm; secondly, he inspected the log-books of all such vessels, as far as was practicable, and carefully transcribed their records; and, thirdly, by an extended correspondence, he obtained a great number of written statements from shipmasters, who of all men would be the most accurate and vigilant observers. The different independent accounts obtained from these various sources amounted to no less than one hundred and sixty-four, all of which were reduced to the form of tables, containing the latitude and longitude of each vessel or place at the time of observation; the exact date and duration of the gale; the successive directions of the storm-wind; the state of the barometer; and, finally, every additional particular, that was deemed of the least importance in determining the peculiar characteristics of the storm. With these data before him, he spread out a marine chart, and having noted on it the position of each vessel and place with the direction and force of the wind, the plot itself proclaimed to the eye the whirlwind character of the storm; and the comparison of dates, and corresponding courses of the winds, and respective states of the barometer, showed the dimensions of the storm, its rotary and progressive velocities, its duration at any given place, and its various degrees of violence at different distances from the centre. In the character of the researches before us, conducted as they were, not in the shades of philosophic retirement and learned leisure, but in hours redeemed from the pressing avocations of an onerous and responsible business, or borrowed from the season allotted to sleep, we trace qualities of mind that belong only to the true philosopher.

The benevolent and practical mind of Redfield had no sooner established the laws of storms than it commenced the inquiry, what rules may be derived from it, to promote the safety of the immense amount of human life and of property that are afloat on the ocean, and exposed continually to the dangers of shipwreck; in this imitating our Franklin, who, as soon as he had discovered the identity of lightning with the electricity of our machines, hastened to the inquiry, How may we so apply our knowledge of the laws of electricity as to disarm the thunderbolt of its terrors? We might pursue the comparison and say, that, as every building saved from the ravages of lightning by the conducting rod is a token both of the sagacity and the benevolence of Franklin, so every vessel saved from the horrors of shipwreck by rules derived from these laws of storms, is a witness to the sagacity and benevolence of Redfield. Other writers on the laws of storms, especially Reid and Piddington, have lent important aid in establishing rules for navigators, until it is now easy for the mariner, by the direction in which the gale strikes the ship, to determine his position in the storm, and the course he must steer in order to escape from its fury. Nor are testimonies wanting of the successful application of these rules. The most accomplished navigators (we might instance, particularly, Commodores Rodgers and Perry, and Commander Glynn, of the U. S. Navy) have testified, that, within their knowledge and in some cases within their own observation, many ships have owed their deliverance from the perils of shipwreck to a faithful observance of the rules derived from Redfield's theory of storms. In no department, perhaps, of the studies of nature, have mankind been more surprised to find things governed by fixed laws, than in the case of the winds. It is now rendered in the highest degree probable, that every breeze that blows is a part of some great system of aerial circulation, and helps to fulfil some grand design. "Inconstant as the winds" has long been a favorite expression to denote the absence of all uniformity or approach to fixed rules; but the researches of the meteorologists of our times force on us the conclusion, that the winds, even in the violent forms of hurricanes and tornadoes, are governed by laws hardly less determinate than those which control the movements of the planets.

It has been often noticed in the history of science and the arts, that great discoveries and inventions spring forth simultaneously from different independent sources. Thus the discovery of oxygen gas, the greatest single discovery in chemistry, was made almost at the same moment by Priestley in England and Scheele in Sweden; and the method of fluxions, or the infinitesimal calculus, was invented at nearly the same time by Newton and Leibnitz. Such discoveries and inventions are the true resultant of innumerable forces, which, at that moment, and never until then, since the origin of time, all conspired. It is remarkable that the idea that great storms are progressive whirlwinds was, for the first time, embraced nearly at the same instant by Redfield and Dové, although the conclusion was arrived at by totally different methods of investigation. Mr. Redfield says, in a note to his paper on the Cuba hurricane, published in 1846, that it was not until seven years after the publication of his theory of the rotary and progressive character of storms, that he became acquainted with the suggestions and opinions of Col. Capper, and with the particular views and elucidations published by Professor Dové in his paper on Barometric Minima, found in Poggendorff's Annalen for 1828. To all who were personally acquainted with Redfield, it would be quite unnecessary to adduce any other evidence than his simple declaration, of the perfectly original and independent character of his theory of the laws of storms. But we might refer to the circumstances under which it was conceived, when he was far removed from all libraries, and all intercourse with the scientific world; and as respects Dové in particular, whose essay was communicated to the public in 1828, it may be said, that at that period there was scarcely a copy of Poggendorff's Annalen (in which Dove's essay appeared) in the United States; and being in the German language, nothing could be more improbable than that its contents were then known to Redfield. In 1838, our friend found, to his great joy, a most able ally in Col. Reid of the Royal English Engineers, then stationed in the island of Barbadoes. The earliest inquiries of Col. Reid were based on a violent hurricane, which occurred in that island in the year 1831. Searching for accounts of previous storms, he met with nothing satisfactory until he fell in with Redfield's earliest paper, respecting the September gale of 1821, published in the American Journal of Science. With the view of testing Redfield's doctrines, he submitted to the closest scrutiny the records which the Barbadoes storm had left of its ravages, —an investigation which ended in a perfect conviction that this storm was a progressive whirlwind. A friendly correspondence was shortly afterwards opened between these two congenial spirits, which resulted in an intimacy, unbroken except by the hand of death. Commodore Perry, in the recent Report of his Japan Expedition, thus expresses himself in an introductory note to Mr. Redfield's Essay (the latest of his published works) on the Cyclones of the Pacific, addressed to Commodore Perry, and forming a part of his volume. "It was my good fortune (says the Commodore) to enjoy, for many years, the friendly acquaintance of one as remarkable for modesty and unassuming pretensions, as for laborious observation and inquiry after knowledge. To him and to Gen. Reid of the Royal Engineers of England (now Governor of Malta) are navigators mainly indebted for the discovery of a law which has already contributed, and will continue to contribute, greatly to the safety of vessels traversing the ocean. It is true that subsequent writers have furnished additional information on this subject; but to Redfield and Reid should be ascribed the credit of the original discovery of this undeniable law of nature, and its application to useful purposes; and there can be nothing more beautiful, as illustrative of the character of these two men, than the fact, well known to myself, that notwithstanding their simultaneous observations and discoveries, in different parts of the world, neither claimed the slightest merit over the other, but each strove to give to his co-worker in research the meed of superior success in the great object of their joint labors; and thus, without ever meeting, a strong friendship was formed between them, growing out of congenial aspirations for an honorable fame, and mutual admiration of the generous and enlightened views exhibited by each other; and this ennobling feeling was kept alive to the last by friendly correspondence."

The idea of whirlwinds is indeed much older than Redfield or Reid, being as old as the writings of the Psalmist and the Prophets; and we safely admit further, that the doctrine of ocean gales being sometimes of a rotary character had been hinted at by several writers, as hints of such a principle as gravitation had long preceded the investigations of Newton; but the honor of having established on satisfactory evidence the rotary and progressive character of ocean storms, and determining their modes of action or laws, it is due alike to the memory of the departed, and to our country's fame, to claim for WILLIAM C. REDFIELD.

Back of the laws that govern these ocean gales, as first determined by Redfield and confirmed by Dové, Reid, Piddington, Thom, and other well-known writers, lies a more profound inquiry, How are these laws themselves to be accounted for? What sets the storm in motion, and gives it the whirlwind character, and at the same time carries it forward, and in so definite a path? What makes it revolve always from right to left on the north side of the equator, and from left to right on the south side? Why does its violence increase towards the centre of the storm, and why is its force there so tremendous? Laws, it must be remembered, are facts, and merely express the modes in which nature acts; they are themselves phenomena to be accounted for. To which of the ultimate causes of physical phenomena is their origin in the present case to be traced? Is it heat? Is it electricity? Is it gravity? Is it connected in some way with the grand system of planetary motion? Questions of this kind were pressed on Mr. Redfield from various sources by those who assailed his theory. At first he declined any attempts at their solution. He claimed that the whirlwind character of storms, and the laws which he had assigned to them, are matters of fact, as established not only by himself, but also by Reid, Milne, Dové, and Piddington; that, never having attempted to establish a theory of winds, nor the origin or first cause of storms, he had no occasion to go into these inquiries, but had long held the proper inquiry to be, *What are storms?* not *How are storms produced?* He, however, incidentally, at different times, indicated his opinions on the ultimate causes of storms. Electricity, Redfield entirely rejected as an agent in the production of winds and storms, considering its presence and development rather as a consequence than as a cause of atmospheric changes. To heat he assigned only a limited and local effect, denying its agency in producing either the great and established movements of the atmosphere, or the extraordinary commotions which constituted the chief objects of his study, hurricanes and tempests. But he considered what he called the "dynamics of the atmosphere" as connected with, and resulting from, the diurnal and annual motions of the earth. While, from the first, I have heartily embraced Redfield's doctrine, that ocean gales are progressive whirlwinds, and have further fully believed that he had established their laws or modes of action on an impregnable basis, a regard to truth and candor obliges me to say, that I have never been a convert to his views respecting the ultimate causes of storms, especially so far as he assigned for these causes what he denominates the "diurnal and orbitual motions of the earth," but his notions on this point have always appeared to me unsatisfactory. Nor, while I have been impressed with the belief that *heat* is, in general, by far the most influential of all natural agents in destroying the equilibrium of the atmosphere, and of causing its motions, both in established currents as the trade winds and the monsoons, and in its violent commotions as in hurricanes and tornadoes, yet I am compelled to think that but little progress has yet been made in determining its *modus operandi*, or in tracing the *connection* between changes of temperature and the actual phenomena of winds and storms:—why, for example, the Atlantic gales originate where they do, in the tropical regions; why they first pursue a path to the north-west as far as the latitude of 30°, and then gracefully wheel in parabolic curves towards the north-east, and pursue this course for the remainder of their way; why they revolve on their axes, and always in one direction; whence they acquire so tremendous a force, especially towards the central parts; why the barometer is so low in the centre and so high on the margin of the storm. These, and various other points connected with the whirlwind character of storms, seem to me to have met hitherto with but a partial and doubtful solution. The laws constitute the true theory of storms; the rest is yet hypothesis.

Various writers have severally displayed great ingenuity and profound knowledge of atmospheric phenomena in their endeavors to solve these problems, but with respect to the causes which lie back of the laws of storms, we still remain to a great degree in ignorance. Each of the combatants appears to me to be more successful in showing the insufficiency of the other's views, than in establishing his own. With respect to him who is more particularly the subject of my remarks, whose logical powers I have always admired, I have almost regretted that he did not adhere to the ground he originally took, namely, that he had not undertaken to explain the reason *why* the winds blow, but only to show *how* they blow. So far was matter of fact; all beyond was hypothesis. His facts are impregnable; his hypothesis doubtful. The conclusions derived legitimately from these facts constitute the laws of storms; and being, as we believe, like the other laws of nature, immutable, the name indissolubly associated with their discovery acquires a fame alike imperishable. Redfield might therefore have safely stopped where Newton stopped. "Newton (says one of his biographers) stopped short at the last fact which he could discover in the solar system — that all bodies were deflected to all other bodies, according to certain regulations of distance and quantity of matter. When told that he had done nothing in philosophy; that he had discovered no cause; and that, to merit any praise, he must show how this deflection was produced; he said, he knew no more than he had told them; that he saw nothing causing this deflection, and was contented with having described it so exactly that a good mathematician could now make tables of the planetary motions as accurate as he pleased, and hoped in a few years to have every purpose of navigation and philosophical curiosity completely answered."

Various other contributions to science of our departed friend must, for want of space, be passed by with hardly a notice.

Such are his published meteorological essays,* his reports of meteorological observations, which contain many original hints of much value, his paper on the currents of the Atlantic, and his researches in geology, which occupied much of his attention during the latter years of his life; all of which speak the skilful observer, the judicious philosopher, the lover of science, the lover of his country and of his kind. His meteorological researches, although they engrossed a large share of the hours he could redeem from the urgent claims of business, did not prevent his taking a strong interest in other branches of science. He attentively watched the progress of knowledge in various departments, but geology had for him special attractions. His powers of observation were early employed, even in his pedestrian tour to Ohio in 1810, in noting facts which appeared to him then to be unaccounted for, but which the progress of the science has since fully explained. In the meetings of the American Association he was an attentive listener to the geological papers, and frequently took part in the discussions which they called forth, exhibiting a thorough acquaintance with the subjects under consideration. The phenomena of the drift period, as evincive of glacial action in various forms, had deeply interested him; and he had collected and closely studied the shells of recent species, which, in the vicinity of New York, are found beneath the deposits of drift. His published geological papers, however, relate chiefly to the sandstones of Connecticut and New Jersey, particularly to their fossils, their ripple-marks, and their rain-drops. His residence in early life was within sight of the extensive quarries of this kind of sandstone, at Portland, Connecticut, and his frequent visits afterwards to that region afforded him opportunity for close observation. In December, 1836, his son, Mr. John H. Redfield, who inherits much of the scientific taste of his father, described † some of the fossil fishes from

* Originally prepared for Blunt's Coast Pilot.

† Annals Lyc. Nat. History, New York, vol. iv.

this locality, and showed that their structural affinities indicated for the so-called "New Red Sandstone" a higher position than had previously been assigned to it. Mr. Redfield pursued the track thus opened by his son, and published in the American Journal of Science descriptions of several new species of Ichthyolites. The last paper which he read before the American Association was upon the geological age of the sandstones of Connecticut and New Jersey, and the cotemporaneous deposits in Virginia and North Carolina. He proposed for them the denomination of the *Newark group*, and showed that the Ichthyolites contained in them pointed unerringly to the Jurassic period. In the course of these investigations he had given close study to the subject of fossil fishes, and had formed a collection of them, probably unequalled in this country, with special reference to a contemplated monograph of the Ichthyolites of the Newark group.

In 1839, Yale College conferred on Mr. Redfield the honorary degree of Master of Arts, and the enlarged sphere in which his labors for the promotion of science and the good of his fellow men were known and appreciated, was evinced by his election into many learned societies in his own and foreign countries.

Three distinguishing marks of the true philosopher met in William C. Redfield, — originality to devise new things; patience to investigate; and logical powers to draw the proper conclusions. The impress of his originality he left, in early life, upon the village where he resided; he afterwards imprinted it still deeper on his professional business, as naval engineer; and most of all on his scientific labors, his observations, and his theories. "Patient thought" was the motto of Newton, and in this attribute Redfield was eminently distinguished. In collecting facts bearing upon his main purpose, and in submitting them to severe and long-continued comparison, he has illustrated this quality in its highest forms, as his laborious investigations of the phenomena of hundreds of storms most fully evince. Originality to invent without patience to investigate, leads to hasty and wild speculations; but united, they lay the deep foundations for a severe logic. His powers of reasoning have always appeared to me to be of high order, and he has been fitly characterized by another eminent writer* on the laws of storms as the "clear-headed" Redfield. Opinions which he had thus formed, after an extensive and patient investigation of the facts and a severe process of reasoning, he held with great tenacity. But though firm, he was not obstinate. *Obstinacy* we define to be an unyielding adherence to our opinions because we have adopted them; *firmness*, a similar adherence to our opinions because we believe them to be right.

Few men have given more signal proofs of an original, inherent love of knowledge. Whether we contemplate the apprentice boy after the toils of the day, seeking for knowledge by the dim light of an open fire; or the father of a young family, through dark scenes of domestic affliction and mournful bereavements, still adding largely, year by year, to his intellectual stores; or the man of business in the whirl of the great metropolis, loaded with onerous and responsible cares, giving every interval of leisure, and the seasons chiefly employed in pleasure or repose, to the study of the laws of nature; or if permitted, as has been my privilege, to be a guest at the house fitted up to be the retreat of his old age, we see the library, the collections of natural history, the many sources of high mental enjoyment, which, in the period gained at last of ease and affluence, distinguish the different apartments of his dwelling; or, finally, whether we call to mind the ever increasing interest with which he attended the meetings of the American Association for the Advancement of Science, and the delight which he experienced in the society of learned men, we observe in all, a mind in love with truth, ever searching

* Reid.

and ever expanding. In society he was courteous, sincere, upright, and benevolent; in his family tender, affectionate, wise in counsel, and pure in example; in all his walk and conversation, and especially in the church of God, a devout and humble Christian.

As the evening of life was passing thus serenely, it hastened to a peaceful close. Mr. Redfield's health had been generally good during his later years, and had seemed particularly so in the early part of the winter which proved his last. On the first of January, he made his usual calls on his friends, and the cheerfulness and vivacity of his manners and healthful expression were never more remarkable. Near the last of January he was seized with alarming symptoms, which indicated effusion in the chest. His disease made rapid and sure progress. The last book which had engaged his attention, previous to his illness, was Dr. Kane's recent Narrative of his Arctic Expedition; and his own feverish dreams, during the earlier nights of his sickness, were confusedly identified with the toils, the difficulties, and the sufferings of that heroic commander and his brave companions. With a general tendency to delirium were mingled intervals of calmness, and throughout his illness his countenance would light up with the smile of affection, as he recognized the relations and friends around him. From the first he entertained but slight hopes of recovery; but as the crisis drew near, his mind was at peace, and in calm resignation to the will of his Maker, and in the full exercise of Christian faith he gently breathed his last.

Happy if we who have so long journeyed with him in the delightful walks of science may enjoy an evening as serene, and find its close as peaceful.

PROCEEDINGS

OF THE

MONTREAL MEETING, 1857.

COMMUNICATIONS.

PART I.

A. MATHEMATICS AND PHYSICS.

I. MATHEMATICS AND ASTRONOMY.

1. ON THE IDEA OF PHYSICAL AND METAPHYSICAL INFINITY. By LIEUT. E. B. HUNT, Corps of Engineers, U. S. A.

FEW subjects of reflection have engaged the meditative energies of so large a portion of the leading intellects of all ages as that great idea which, under a vast diversity of forms and manifestations, is expressed by the word infinity. So true is this, that the charge of rash confidence might well be preferred against whoever should now profess to contribute any great additional light, where so much thinking has already been expended. In spite of this presumption, I shall venture some suggestions towards a precise definition of the idea of infinity, which have served to make clearer, to my own mind, what before was vague and indefinite.

It has seemed to me a correct criticism on the usual modes of considering the subject of infinity, that they regard it too exclusively under its metaphysical or speculative aspects, and too little in its physical or actualized forms. By at once pushing the idea of infinity into its abstract phases, we banish it from our positive cognizance, and relinquish the aids which nature affords in interpreting it to our finite comprehension. That such a hasty transfer from the concrete to the abstract form of contemplation involves a fault, may be appreciated at once by a simple consideration, in which all healthful minds will doubtless agree. The idea of infinity must dwell in the Divine creative mind in its greatest supposable perfection; consequently, its natural embodiments must possess a wholeness and intellectual truth, far exceeding what could originate from the abstract speculations of human mind. Therefore, whatever hints towards a due appreciation of infinity can be gleaned from the contemplation of actual created nature, will rest on a more solid basis than any unguided speculations can claim.

Whatever is intrinsically measurable may, by continuous quantitative expansion or diminution, grow to such an incomprehensible magnitude, that infinity becomes predicable of its value. Thus time, distance, space, number, force, or any quality of matter or mind, as hardness, temperature, light-producing power, sensational perceptive capacity, intellectual comprehension, force of will, benevolence, veneration, or, indeed, any species of actuality which can be regarded quantitatively, may be supposed so great or so small that the human mind will call it infinitely great or infinitely small. No such predicate would ever be used in speaking of what was not intrinsically measurable. Intrinsic mensurability is, therefore, a fundamental preliminary to any concrete infinity. In other words, whatever is infinite must have a unit of measure, and its infinitude consists in the relation between its aggregate quantity and its unit of quantity. Physical infinities thus fundamentally involve homogeneous physical units of measure as standards of reference. Therefore, the apprehension of infinity involves an apprehension of unity as its initial point, and this is equally true, whether the magnitude, called infinitely great or small, is physical and actual, or metaphysical, and only abstractly conceived. Hence our first necessary step is to analyze the idea of a unit of measure.

When we speak of a foot, a cubic yard, a pound, an hour, a thermometer-degree, etc., the ideas expressed are among the clearest which the human mind can entertain. Our mental and moral qualities, though intrinsically capable of equally exact units of measure, being actually without well-defined units, their quantitative comparisons become vague, and wanting in precision. Yet we are quite as prone to speak of Infinite intelligence or Infinite love, as of infinite distance or infinite time. This mode of speech rests on precisely as real a unit of measure as in the strictly physical cases, though it is less accurately defined to our own minds.

If then we take a single instance of a unit of measure for close analysis, the general results of such a discussion will reach to all the analogous cases.

Taking the unit of linear measure as this instance, we find that the original standards among all nations, whence other units of length are derived, have a pretty close general agreement. The foot, the yard, the metre, the toise, etc., are all evident derivatives from the human body, and stand in close relations to certain convenient modes of measurement, by reference to the geometry of the body. Our optical perception of perspective distances involves an habitual process, — first, of reference of all dimensions seen, to those distances near at hand, which are readily comprehended, and, in turn, of the reference of these to the actual linear distance between the optical centres of the two eyes. This interocular distance, or stereoscopic base line, bears the same relation to exterior visible distances, that the base line, in a geodetic triangulation, does to the entire network of triangles. Thus, when we gauge the perspective of a landscape, there is a direct visual perception or sensational measurement of external distances; — still, we are so habituated to this stereoscopic function of the interocular base line, that we do not make it an object of conscious contemplation in our reference of external distances to this base as a unit of measure. Such a reference, however, really enters as a vital part of every perspective perception. Hence we are constantly applying, unawares, a standard measure essentially unchanged for each individual during his whole life, to all external objects of our earthly surroundings. In like manner, the length of our habitual step enters largely as a basis in our estimation of distances, because we are constantly measuring distances seen by our habitual mechanism of locomotion. Thus, all our means of knowing external distances are found at last to rest solely on the actual dimensions of the human body, to which, as a standard, they are at last referred.

Taking into account all the elements of our perception of linear magnitudes, it will be found, without doubt, that the average linear unit is very nearly that length which is most readily cognizable by a man of average person and capacities. High multiples and low submultiples of the standard of length are difficult of appreciation; thus — a mile and a line are much less clearly apprehended units than a foot. If a thousand miles or a thousandth of a line be submitted to our consciousness, our notion becomes extremely inadequate, and if it be a million or a millionth, we fail almost entirely to conceive the fact. When it is a question of billions or trillions of miles, our apprehension is so totally at fault, that we give over all attempts to comprehend the relation, and so the distance becomes infinite for us, when referred to the mile as a unit. Thus our real idea is, when we speak of an infinite or infinitesimal distance, that when it is compared with our familiar standard of length, our perceptive powers utterly fail to appreciate the relation with any approach to accuracy.

If in place of the unity and infinity of distance we consider those of time, space, or force, we shall find a like genesis of practical standard units for each, based on the actual dimensions or sensational capacities of the human organism. Along the graduated line of connection between those values which by their minuteness utterly elude our perception, and those vast values which in their entirety wholly transcend our comprehension, there is, in each case, a particular value, which is apprehended with the maximum precision and facility by each individual mind. The natural unit of measure for each subject of measure is that particular value which is best apprehended by an average man. The same rule holds in the more transcendental subjects of measure; thus — a man of average intellectual power and capacity becomes a natural unit of mentality, and a man of average morality becomes a unit of morality. If a particular moral quality as benevolence, for instance, be quantitatively considered, we refer all to the man of average benevolence. When we speak of Divine benevolence as infinite, we mean that it is so exhaustless and all prevailing, that when we compare it with the benevolence of an average man, our limited human powers utterly fail to take in its relative immensity. From these considerations, we may conclude that whenever we predicate infinity of any particular existence, attribute, or quality of the external or internal world, man's capacity to appreciate the various values of the subject-matter considered enters directly as the standard of comparison. Thus, to say that space is infinite, is simply to say that the extreme exercise of human power to conceive space, as an existence, is transcended by the actuality of nature. If we speak of infinite time, we but declare that the brief periods of duration, of which man in his earthly life is conscious, are relatively so small that we can by no means conceive the number expressing the true ratio of man's hour or lifetime to the infinite duration referred to. Whatever physical infinity engages our consideration, an analogous limitation of the special powers of the human organism is defined. We might almost say that, for us, the grand sphere of physical infinity is the circumscribing sphere drawn around the aggregate perceptive faculties of man. It is not, at all, the absolute cosmos or circumscribing sphere which contains all the actualities of nature. We may well believe that this true cosmic sphere, in which all created existence is contained, is itself an infinity, as compared with that specific infinity which is, as it were, the defining or tangent surface around the faculties of man.

It is entirely supposable that among the actual organic existences of nature there may be numerous successive grades of perceptive capacity, which stand in the relation of coterminous or successive infinities as judged by each other. A monad may have a direct perception of man's infinitely small, and our sensible distance must be to it an infinitely great, magnitude. There may be intelligences such that the radius of an ultimate atom of matter would be to them what the radius of the earth is to us, as there may be intelligences to which the earth is but an atom, and to which our entire sphere of visible stars makes but a sensible mass of matter. Throughout the entire range of organic existence there will be, in fact, for each species a specific infinity, and we cannot say but in the treasure-house of the actual universe there may be an infinite series of organic perceptive powers, which bear to each other the relation of the successive orders of differences in differential calculus. Whatever may be the fact as to actual nature, such an infinite series of successive infinities is metaphysically conceivable. The clear apprehension of the idea of infinity which may be gleaned from physical grounds gives a basis for indefinite metaphysical fabrications, without in the least departing from the true, inductive idea of infinity. But science has not to deal with the supposable, except as it is involved in the actual, and it belongs not to this place or to true philosophy to go beyond the foundations of fact.

The views now presented have a bearing on the mathematical idea of infinity, which is not without importance. The mathematical symbol of infinity stands for an entirely abstract idea. From it all the definite standards of unity which have been discussed are entirely eliminated. All specific intelligences are in it ignored, and even the Divine Intelligence may be supposed in some way concrete in conditions too specific to be truly stated in respect to limits by the bald abstract infinity of pure analysis. The very possibility of positive definition, as applied to any being, however exalted, excludes the abstract symbol of infinity from entering a correct exegesis of its nature. For what, then, does the abstract symbol of infinity stand? It at least stands as a formula for all specific infinities, which, by the interpolation of the proper constants, expresses the quantitative relations in any actual case of infinity. The abstract symbol is a grouping of specific cases; whether it is more than this may not be for man to say.

One inference from these views of infinity is, that the ordinary definition of an asymptotic curve needs correction. The mathematical formula of incessant and incessantly diminishing approach between a straight line and a curve, or between two curved lines, or the same relative to plane and curved surfaces, is not consistent with the other idea of tangency at an infinite distance. Suppose an intellect of the proper or differential grade to be duly cognizant of asymptotic lines at an infinite distance, it would find no actual contact, but the same law of approach expressed in the analytical formula would still go on, until an intellect of the second differential order would have to be called in as the cognizant power; this, in turn, must give place to a third differential intellect, and so on to infinity. The order of perception required to appreciate the second, third, etc., differentials of the function, would be progressively higher than that demanded for the differential of the variable, which is supposed to be always constant. Here there is no true tangency, but a perpetually decreasing approximation. Hence the definition of asymptotes should give the idea of a perpetually diminishing and never ending approximation, instead of involving the false notion of tangency anywhere. The geometrical method of exhaustions escapes any such criticism, and in the clearest manner embodies the true conception of infinity.

As might be expected, the infinitesimal calculus expresses the notion of infinity in its most precise and purified form. The fundamental idea on which its processes and algorithm rest is one of relation between quantities cognizable by two orders of perceptive intelligence, so remote that the finite quantity of the one is the infinitely great or small of the other. By the hypothecation of three, four, or more coterminous orders of perceptive faculty, the second, third, etc. orders of differences are philosophically originated. The relations of differential calculus, if inversely stated, become those of integral calculus, and fall under the same generalizations relative to orders of perceptive capacity. I will venture the suggestion, that the reason why some eminent mathematicians have contended that a differential is absolutely zero, is simply because of their not having regarded, as they clearly are bound to do, the element of limited perceptive faculties which infinity involves, and the consequently supposable series of cognitions precisely conforming with first, second, third, etc. differentials. It is the same fault of conception which invalidates the definition of asymptotes based on tangency at an infinite distance.

In conclusion, we may lay down the general proposition, that the idea of infinity involves in all cases, as an essential factor in its composition, a specific, actual, or hypothetical limitation of perceptive power in the intelligence, of whose cognition infinity is predicated. In the physical infinities which chiefly interest us, the limitations involved are, *for us*, altogether those which encompass the mind of man. We can, by hypothesis, suppose other limits conformed to other grades of organism, and we can even suppose such an exalted and spiritual organism as that the absolute and entire cosmos shall be the true limit of its perceptive powers. Before the Divine mind, this cosmos must stand in that clear, finite relation necessarily supposed between a creator and the thing created. Beyond the actual cosmos, there may be an immensity of possibility, where the Divine mind may realize analogous limitations to those which hedge in all created minds. In the mathematical or purely abstract idea of infinity there seems a suggestion of such a possibility; and when we consider that a mathematical formula is the nearest possible approach to a literally Divine thought, we shall bow with reverence before the suggestion shadowed forth by the sublime symbol of infinity, after all created limitations are eliminated from its significance. This symbol affords no basis for the commonly received idea that infinity means an *absolute unboundedness*, a quantity *absolutely* without end, a quality or nature *absolutely* transcending all boundaries. Such an idea has no right in the mind of man, for the limitations of human perception forbid our attainment of any knowledge, either of the extent of the absolute cosmos, or of the boundaries around what is abstractly possible. The formula of infinity, so far from stating an absolute boundlessness, endlessness, or illimitable magnitude, states simply the limitations of finite perceptive power. For us it is the expression, not of the immeasurableness of nature or of the Deity, but of the finite limitations of the human mind. It stands for a negative, and not for a positive; it symbolizes, not knowledge, but ignorance. If we group infinite attributes under a Divine name, we have not defined Deity, but we have defined the limits of our own perceptions. The limits of our knowledge lie near at hand; the limits of our ignorance are known only to the All-knowing.

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2. Systems of Coördinates in one Plane. By Thomas Hill, Waltham, Mass.

If Mm be an infinitesimal portion of a curve, the position of M may be defined by means of

x, y, the rectangular coördinates,

or r, φ, the polar coördinates.

The character of the element, Mm, may be defined by means of

ε, the angle which r makes with the curve.

τ, the angle which the curve makes with the axis of x.

ϱ, the radius of curvature.

ds, the element Mm of length in the curve.

The nature of the curve, of which Mm is an element, may be defined by an algebraic equation between any two of the quantities, x, y, r, φ, ε, τ, r, ϱ, s. If either of the quantities, x, y, and r, is used, the position, as well as the form, of the curve is usually fixed. If φ or ε enters the equation, the position *may* be fixed. An equation embracing only two of the quantities, τ, r, ϱ, s, although defining the form and sometimes the size of the curve, and even its position with reference to a fixed direction, cannot define its position with reference to any fixed point.

From these quantities twenty-two different couples may be selected, and either member of each couple may be taken as the independent variable, and the other considered as a function thereof. Thus we have, in one sense, forty-three general systems of coördinates, by which we may represent a curve without the use of any thing more than ordinary algebra. About one fourth of these systems appear, to me, to be worthy of investigation. Rectangular coördinates in which y is a function of x, and polar, in which r is considered a function of φ, are too well known to need any particular remarks. The first has the advantage over all others, in its generality and flexibility. The second is peculiarly adapted to curves of circular symmetry. Both systems have also this advantage, that an equation in them is readily constructed upon paper; an advantage which would, however, be retained in some other systems; for instance, in one representing x as a function of φ. But all those systems which refer to a fixed origin, have also the disadvantage of not showing readily the evolutes of the curve, and also of being confined in their range to the first order of infinity.

Algebraical language, even under the forms of Hamilton's Quaternions, is an imperfect means of presenting geometrical truths, and although an equation between any two of these quantities will give a law of the curve, yet no two equations will give precisely the same view, even if they give the same law. Both the denotation and the connotation of the language vary in every transformation of the equation. I have illustrated this truth by throwing the equilateral hyperbola into seventeen systems of coördinates, and comparing the geometrical interpretation of the equations; and by throwing the parabola, the cycloids, and other curves, into six or seven systems. The variation of geometrical meaning is very striking.

The cycloid, for example, is, by most systems, represented as generated by a rolling circle, and consisting of an indefinite number of arches. But represented by an equation between ϱ and s, $\left(\varrho = (A^2 - s^2)^{\frac{1}{2}}\right)$, the cycloid becomes finite in length. If ϱ is represented as a function of s, the cycloid is a single arch; if s is considered as a function of ϱ, the cycloid is two half arches including a cusp. All the rest becomes imaginary, thus strikingly demonstrating that the imaginary may be real, since the real may become imaginary.

On the other hand the parabola is usually considered as a conic section, consisting necessarily of a single arch. But an equation of the parabola from which x, y, and r are eliminated,

$$\left(\cot \varepsilon = \cot \tfrac{1}{2}\,\varphi,\ \tau = \tfrac{1}{2}\,\varphi,\ \varrho = \frac{2\,\mathrm{p}}{\cos^3 \tau}\right)$$

will sometimes give us a series of arches like the cycloid,— the arches being infinitely deep and infinitely wide.

If we define an equilateral hyperbola as a curve, the rectangle of whose ordinate and abscissa is constant, we evidently make it consist of four branches represented in rectangular coördinates only by means of the two equations,

$$xy = \sqrt{A^4}.$$

Using the positive root of the second member would give only the right upper and left lower branch. Nine of the seventeen systems unite in preferring these two branches, but differ in the points corresponding to zero of the variable; that is, some begin the branches at one end, and others at the other. Seven systems give us the whole of the four branches, some beginning the branches at one end, some at the other, one at both ends at once, and one at the middle of each branch, running out both ways at once. Two of these systems ($\sin \varepsilon = \sin 2\,\varphi$), ($\tan \varphi = \tan \tau$) give us the curve of four branches, but without defining its size. These equations are equations of pure form, independent of magnitude. Some of these sixteen equations, while defining the form and size, and even the distance from a fixed origin, do not define the direction. They are equations of a circle with a hyperbola, or an indefinite number of equal hyperbolas tangent upon it.

The seventeenth equation, by which I have represented the equilateral hyperbola, does, in fact, represent the whole plane; it is,

$$\varrho = \frac{r^3}{2\,A^2}.$$

But by fixing the arbitrary constants, and drawing some of the curves, it will be found to embrace not only the hyperbola, but other forms.

In like manner $\varrho = Ar$ will give not only logarithmic spirals, but two other forms of new curves.

These results are obtained by the ordinary processes of analytical geometry, including the calculus, without any attempt at the interpretation of imaginary quantities, or any extension of notation in the manner of the Quaternions. They are sufficient to show how much lies concealed in even the ordinary walks of the geometer, and how much may be discovered in geometrical truth by the mere metamorphosis of algebraic notation.

Some of these systems of coördinates appear to me to deserve a more thorough investigation than they have as yet received. They appear to be as natural and as comprehensive as either of the three systems now in use. If, for example, the direction of a curve is represented as a function of its length $(\tau = f(s))$, or its radius of curvature as a function of its length $(\varrho = f(s))$, you have a very natural and useful mode of defining the curve. Good results are also to be obtained by making φ a function of s. Since arriving at this meeting I have learned that Whewell's "intrinsic equations" of a curve, which I had supposed identical with "Peirce's circular coördinates," are formed between τ and s.

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3. On the Warped Surfaces of Ground occurring in Road Excavations and Embankments. By Prof. W. M. Gillespie, of Union College, Schenectady, N. Y.

When an engineer is laying out a road or railway, and has to determine the amount of earth necessary to be moved in making the "cuts" and "fills," he takes "Cross-sections," or "Profiles," of the ground at right angles to the line of road at convenient intervals, and then calculates by various methods, usually near approximations, the volume included between each pair of these cross-sections. The distances apart at which these cross-sections are taken are determined by the engineer according to the nature of the ground, his aim being that there shall not merely be no abrupt change of height between each pair of these cross-sections, but that the surface from one to the other shall *vary uniformly*; gradually passing, for example, from a small to a great degree of slope, or from a slope to the right into a slope to the left, without any sudden variation at any one place.

The surface fulfilling this condition of "varying uniformly," since it is everywhere straight in some direction, is evidently a *ruled surface;* and since the extreme profiles are seldom parallel, it will generally be a *warped* or *twisted* surface.

Our engineers have been accustomed to consider these surfaces as not admitting of precise calculation, but only of a degree of approximation varying with the nearness of the cross-sections.* The object of this paper is to examine the correctness of this position. It will therefore have two parts: firstly, a discussion of the precise nature of the surface; and secondly, an investigation of a formula applying to it.

I. *What sort of a warped surface is the one in question? that is, what is its mode of generation?*

To determine this, we must inquire what the engineer means when he says that the ground "varies uniformly" from the place at which he stands, and at which he has just taken a cross-section, to the place at which he decides it will be proper to take the next cross-section; whether he means that the ground between the two is straight lengthwise or straight crosswise — straight in the direction in which the road runs, or straight at right angles to that direction.

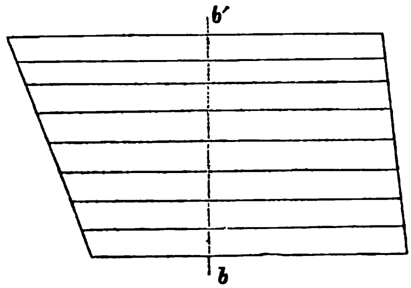
Probably few engineers ask themselves this question in so many words; but it would seem that the latter conception, or *straightness crosswise*, is the more likely to be what is meant, for the reason that any deviation from straightness in that direction, at right angles to the line along which we look, is much more easily seen than in the other direction. We can therefore much more readily determine whether the surface of the road is straight or curved from side to side than from end to end, and the surface which we pronounce uniform is therefore much more likely to be straight crosswise than lengthwise.

In geometrical language, the former surface (shown in plan in fig. 1) is generated by a straight line resting on the two straight lines which join the extremeties of the two profiles, and moving parallel to

* Thus the preface of a recent "Table, etc.," says: "No practicable method of calculation will give a true result, if the surface of the ground is much warped."

their planes, or perpendicular to the axis of the road. This surface is a "HYPERBOLIC PARABOLOID."

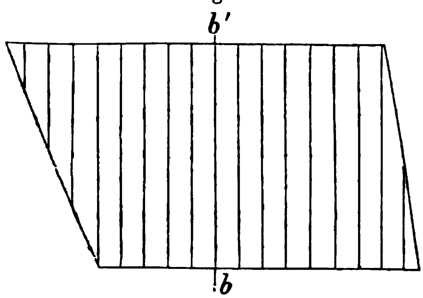
Fig. 1.



The latter surface (fig. 2) is generated by a straight line resting on the two profiles, and moving parallel to the vertical plane which passes through the axis of the road. It also is a hyperbolic paraboloid, though a different one from the former.

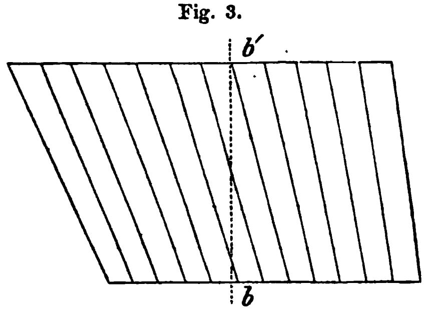
The French engineers adopt the latter hypothesis. We have seen, however, that the former is the more probable one.

Fig. 2.



But fortunately the difference is really very slight; for a very small change in the latter hypothesis will make its result identical with that of the former. Conceive the straight line which rests on the two profiles to move on them in such a way as always to divide them *proportionally*, as in fig. 3. The surface thus generated is *identical* with that of fig. 1, as is proven in the treatises on Descriptive Geometry.*

Fig. 3.



This last conception is also more probably correct than that of fig. 2, even if we suppose the engineer to consider longitudinal straightness; since he is more likely to extend his imagination

* Olivier, p. 263, etc.; Leroy, p. 249.

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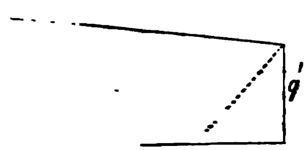

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... [1]

Arranging this expression according to the ascending powers of x, it becomes

$$\tfrac{1}{2}\left[b\,(p+q)+\frac{b\,(p'-p+q'-q)+(b'-b)\,(p+q)}{l}\,x\right.$$
$$\left.+\frac{(b'-b)\,(p'-p+q'-q)}{l^2}\,x^2\right].$$

The product of this by dx being the differential of the solid, its volume will be

$$\int_0^l \tfrac{1}{2}\left[b\,(p+q)\,dx+\frac{b\,(p'-p+q'-q)+(b'-b)\,(p+q)}{l}\,x\,dx\right.$$
$$\left.+\frac{(b'-b)\,(p'-p+q'-q)}{l^2}\,x^2\,dx\right]. \qquad [2]$$

Integrating, we obtain the expression

$$\tfrac{1}{2}\left[b\,(p+q)\,l+\tfrac{1}{2}\,b\,(p'-p+q'-q)\,l+\tfrac{1}{2}\,(b'-b)\,(p+q)\,l\right.$$
$$\left.+\tfrac{1}{3}\,(b'-b)\,(p'-p+q'-q)\,l\right].$$

Performing the operations indicated and factoring, we finally obtain, as the volume of the solid, the symmetrical expression

$$\tfrac{1}{6}\,l\left[(b+\tfrac{1}{2}\,b')\,(p+q)+(b'+\tfrac{1}{2}\,b)\,(p'+q')\right].^{*} \qquad [3]$$

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* Two particular cases of this general formula are worthy of special notice.

Let the base of the given solid be a parallelogram.

Then $b=b'$, and formula [3] becomes

$$\tfrac{1}{6}\,l\left[\tfrac{3}{2}\,b\,(p+q)+\tfrac{3}{2}\,b\,(p'+q')\right]=b\,l\times\tfrac{1}{4}\,(p+q+p'+q').$$

That is, the volume of a right prism, having its base a parallelogram, and its upper surface warped, equals the product of its base by one fourth of the sum of its edges or heights.

Next, let the base be a triangle.

Then $b'=o$, and $p'=q'$, and formula [3] becomes

$$\tfrac{1}{6}\;l\;\left[b\,(p+q)+b\,p'\right]=\tfrac{1}{2}\,b\;l\;\times\tfrac{1}{3}\;(p+q+p')].$$

That is, the volume of a triangular prism, with its upper surface warped, equals the product of its base by one third of the sum of its edges or heights.

The above two rules have been already obtained by the French geometers, by the aid of other considerations derived from the Higher Descriptive Geometry.

Of course they are also true when the upper surface of the prism is a plane, since a plane is only a particular case of a hyperbolic paraboloid. They thus give a general proof of the well-known rules for the content of truncated prisms having triangles or parallelograms for bases.

We now propose to show that the volume given by the preceding formula [3] is the same as would be obtained by applying the familiar prismoidal formula to the given solid.

The area of the section $A = \frac{1}{2} b (p + q)$.

The area of the section $B = \frac{1}{2} b' (p' + q')$.

The area of the section midway between A and B equals

$$\tfrac{1}{2} \left\{ \tfrac{1}{2} (b + b') \times \left[\tfrac{1}{2} (p + p') + \tfrac{1}{2} (q + q') \right] \right\}.$$

Adding together the areas of A and B, and four times the middle area, and multiplying the sum by $\frac{1}{6} l$, we obtain

$$\tfrac{1}{6} l \left(bp + bq + b'p' + b'q' + \tfrac{1}{2} bp' + \tfrac{1}{2} bq' + \tfrac{1}{2} b'p + \tfrac{1}{2} b'q \right),$$

which can be decomposed into the following:

$$\tfrac{1}{6} l \left[(b + \tfrac{1}{2} b') (p + q) + (b' + \tfrac{1}{2} b) (p' + q') \right], \qquad [3']$$

an expression identical with the general formula [3].

We thus arrive at the conclusion that the familiar "prismoidal formula" can be applied with perfect accuracy to such solids as we have discussed, having one of their faces a warped surface, generated as in our first (or third) hypothesis.

We have thus far supposed that the road-bed was level, or, in more general terms, that the face of the solid was perpendicular to its ends. It may, however, make oblique angles with them. But the prismoidal rule will still apply to the new solid thus produced, since it applies to the wedge-shaped piece, by the removal of which the original solid can be converted into the new one, and therefore applies to the new one, which is the difference of the two; seeing that all the areas involved enter into the formula only by addition or subtraction, and have a common multiplier.

We have also supposed the sides of the solid to be vertical; but in road excavations, etc., they usually slope, as shown by the dotted lines in figs. 4 and 5. But the prismoidal rule still applies, for the same reasons as in the preceding paragraphs, since it applies to the pyramidal frusta whose removal changes the original solid into the one now proposed.

This rule also applies to the "*three-level ground*," familiar to engineers, in which the centre level and the outside levels of a cross-section are given. A vertical plane passed through the axis of the road divides it into two solids, such as we have been considering. It can, therefore, be correctly calculated as one prismoid.

The rule may also be applied to "*irregular cross-sections*," or those in which the surface of the ground is so irregular that a number of levels have to be taken at various points of each profile. To do this, we must conceive vertical planes to pass through all the points of each profile at which the transverse slope of the ground changes. They will thus divide the solid into a number of solids with warped surfaces, such as we have been considering. The principle of our third hypothesis will then enable us to determine by a simple proportion the height at which the vertical plane passing through any angular summit of one profile cuts the top line of the other. This will furnish the data for applying the preceding rule.

If the views here presented should meet with general acceptance, engineers would be enabled to economize much time and labor, since they would no longer feel compelled, as now, to take their cross-sections so near together that the ground between them should be approximately plane, but could take them as far apart as the ground *varied uniformly*, no matter how much, or for what distance, that might be.

The adoption of these principles would also seriously modify the methods now employed in the calculation of earthwork, and the results obtained by them, particularly the use of "equivalent mean heights." The numerical details of these professional matters would, however, be out of place here.

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4. On the Law of Human Mortality that appears to obtain in Massachusetts, with Tables of practical value deduced therefrom. By E. B. Elliott, of Boston.

The accompanying tables comprise part of an original series, that has been prepared for the New England Mutual Life Insurance Company of Boston, from extensive and reliable European and American data. They have been calculated from official abstracts of observations made for the year 1855, respecting the status and the movements of the population in the 166 of the 331 towns of the Commonwealth of Massachusetts, in each of which, the ratio of the number of registered deaths, to the number of the population, was *greater* than one to sixty-three.

With the numbers returned for the 166 towns, have been included two thirds of the numbers of the population, births, and deaths of the three State almshouses; the population of the 166 towns being two thirds (.663) of the population of the entire State.

The aggregate population of these communities, as returned for the first day of June, 1855, was 751,241, and the registered deaths in these towns during the year was 16,086. The well-known Carlisle table of mortality was deduced from only 1,840 deaths, registered during the nine years 1779–87, the mean population of the period being 8,177.

The rate of mortality, or the ratio of deaths to the population, according to the returns, was probably somewhat lower, in these communities, than the rate that actually prevailed in them, in consequence of probable omissions in the registration of deaths in some of the districts.

But these communities embrace a larger proportion of the more populous districts of the State, and of those in which we should expect the prevailing rate of mortality to be higher than the rate for the entire State; and we are probably safe in concluding that the law of mortality obtaining in these districts, according to the returns, does not greatly vary from the law of mortality actually prevailing over the entire population of the Commonwealth.

It is not possible, supplied with only our present information, to indicate the precise line of separation between the reliable and the questionable data; but it is thought that such a division has been made that the data retained are affected by only inconsiderable errors, and the errors of excess and of defect nearly compensate for each other.

The tables deduced from the resulting law of mortality are intended to facilitate the solution of certain problems in political arithmetic, and to furnish replies to questions involving the probable duration of human life.

Tables are also given, comparing the rates of mortality obtaining in Massachusetts at divers intervals of age, with corresponding rates obtaining in certain European communities.

The "Fourteenth Annual Report relating to the Registry and Returns of Births, Marriages, and Deaths, in Massachusetts, for the Year 1855," and the "Abstract of the Census of the Commonwealth of Massachusetts, taken with reference to facts existing on the first day of June, 1855," prepared under the direction of the Hon. Francis DeWitt, Secretary of the Commonwealth, and under the supervision of Dr. Nathaniel B. Shurtleff, of Boston, appear to have been the first and the only official reports, whether State or national, in which either the ages of the persons living, or the ages of those dying, in the State, have been *distinguished by towns;* consequently, they are the first that furnish data, from comparison of which it has been possible to construct a Life Table that can satisfactorily express the law of mortality prevailing over any considerable portion of the people of the Commonwealth.* † In previous reports the ages have been distinguished only *by counties.*

Since the Registration Act of 1849, the registrars, in many of the towns of the Commonwealth, appear to have annually furnished the office of Secretary of State with very valuable and accurate statistics respecting the births, marriages, and deaths occurring in their respective districts.

In other towns the results indicate that this information has been but imperfectly recorded, while in a few cases the officers have uniformly neglected either to register or report.

In registration reports, previous to that for the year 1855, the full

* A Life Table, prepared by the eminent Dr. Edward Wigglesworth from sixty-two Bills of Mortality recorded previous to the year 1789 in the States of Massachusetts and of New Hampshire, was published in the second volume of the Memoirs of the American Academy of Arts and Sciences. Unfortunately, in constructing the table, allowance was not made for the fact that the population had been rapidly increasing, and the table was framed on the assumption that it had been stationary. This table has been employed by the courts of the Commonwealth in determining the values of "life interests in estates, legacies, and pensions," and the values of "reversions in heritable property."

† A valuable Life Table, constructed from observations respecting the mortality of the Alumni of Harvard University, and, consequently, expressing the law of mortality which prevails, in America, over the more highly educated classes, was laid before the American Association at its late meeting in Montreal, by Prof. Benjamin Peirce, of Cambridge.

and reliable information respecting the ages of the dying furnished by the able and competent registrars, in certain towns, has been vitiated by union with the questionable or obviously defective data obtained from other towns in the same county.

In the Registration Report for 1855, the table which distinguishes by age, sex, and locality the deaths registered during the year (Table VI.), was prepared with special reference to its employment in the construction of Life Tables, and is believed to be a faithful abstract from the returns.

The enumeration of the numbers and ages of the population of Massachusetts, according to the State Census of 1855, and the previous enumerations ordered by the General Government, may safely be regarded as reliable.

It is to be regretted that the abstracts of the Census for 1855, in giving the ages of the population, did not distinguish the *sexes*.

This deficiency may, in a measure, be supplied by assuming that the proportional distribution of the sexes at the different ages in 1855, was the same as that obtaining in Massachusetts in 1850, according to the National Census of that year.

Of the 324 statistical districts into which England and Wales are subdivided, in only *two* was there indicated, according to very accurate observations for the seven years, 1838–44, an annual rate of mortality *less* than one death to sixty-three persons living. Of the 331 towns in Massachusetts (nearly the same in number with the English districts just mentioned), there were, in 1855, 165 towns, in each of which the rate of mortality, indicated by the returns, was *less* than one to sixty-three. The returns from the three State almshouses are not included with the returns of the towns in which they happen to be located, namely, Monson, Tewksbury, and Bridgewater. The average population of the 331 towns of Massachusetts is much less than that of the 324 English districts; the population of Massachusetts, in 1855, being one million (1,132,369), and that of England and Wales, in 1841, sixteen millions (15,927,867). The above assumed test, in selecting returns showing a mortality over one to sixty-three, is accordingly confirmed by the English returns, which are made, as is well known, with great accuracy.

The mortality of the 324 English districts varied from 1 in 70 to 1 in 30; in the 331 towns of Massachusetts, according to the returns, from *zero* to 1 in 30. The mortality of Massachusetts, according to the entire returns, 1.84 per cent., or 1 in 54; in the 166 towns it was 2.14 per cent., or 1 in 47.

AVERAGE ANNUAL RATES OF MORTALITY OBTAINING IN THE WHOLE, OR IN PARTS OF SEVEN COUNTRIES OF EUROPE, ARRANGED IN THE INVERSE ORDER OF THEIR RESPECTIVE INTENSITIES.

Years in which the Deaths occurred.	Countries.	To 100 Persons Living, Deaths.	One Death To Pers'ns Living.
7 years 1838–44	England and Wales *	2.19	46.
20 " 1821–40	Sweden †	2.34	43.
4 " 1839–42	France ‡	2.36	42.
9 " 1842–50	Belgium §	2.42	41.
3 " 1839–41	Prussia ‡	2.70	37.
3 " 1834, 7, 9	Austria ‡	3.09	32.
1 " 1842	Russia ‡	3.59	28.

According to the above, it appears that in England and Wales the conditions are most favorable to vitality, and in Russia, least; the several countries, arranged in the inverse order of the respective intensities of mortality, being England, Sweden, France, Belgium, Prussia, Austria, and Russia.

The population of Massachusetts has increased more rapidly than those of the principal countries of Europe. The annual rate of increase of the former, during the years 1850–55, was two and two thirds per cent. (2.63). The effect of an increase by births merely is to diminish, in some degree, the general rate of mortality, even though the intensity of mortality, at different ages, remains unchanged.

We will now proceed to the construction of the Life Table.

* 9th Report Registrar General (England).

† 8th Report Registrar General (England).

‡ See 6th Report Registrar General (England).

§ Statistique de la Belgique, Publié par le Ministre de l'Interieur.

Table I.—Number and Ages of the Population of Massachusetts, according to the National Census of 1850 and of the State Census of 1855; also the Annual Ratio of Increase, and the Logarithms of the Monthly Ratio of Increase, the Ratios being corrected for the Numbers Returned at Ages not specified.

Ages.	Population. 1850.	Population. 1855.	Unity plus the Annual Rate of Increase.	Logarithms of the Monthly Ratio of Increase.
0–1	23,192			
1–5	90,853			
0–5	114,045	132,944	1.03131	.0011159
5–10	102,797	115,862	1.02439	.0008720
10–15	98,024	110,098	1.02367	.0008468
15–20	105,741	117,047	1.02069	.0007413
20–30	210,997	235,678	1.02254	.0008067
30–40	143,931	165,046	1.02793	.0009968
40–50	96,266	111,500	1.02999	.0010694
50–60	60,254	71,829	1.03594	.0012779
60–70	36,837	42,423	1.02881	.0010279
70–80	17,936	20,810	1.02034	.0010818
80–90	5,820	6,138	1.01086	.0003911
90–100	613	634	1.00693	.0002498
100 and upwards	19	19	1.00017	.0000060
Age not specified	1,234	2,341		
All ages	994,514	1,132,369	1.02630	.0009396
Specified ages	993,280	1,130,028		
90 and upwards	632	653		.0002426

From the returns of births for the six years, 1850–55, it appears that the annual rate of increase was 3.49 per cent. But the annual rate of the increase of the living, under the age of five years, was only 3.13. The latter is believed to be the more correct. The difference is probably owing to a gradual improvement in the completeness of the returns of the births.

In that which follows, we shall assume that the population in the selected districts increased, at the different intervals of age, at the same rates as in the entire State.

The population of the 166 towns may be divided into two classes, the *migratory* and the *permanent;* the former comprising immigrants and emigrants.

We assume that the proportional distribution of the ages of those living under the age of *five* years, was the same in the latter as in the former class; that the ratio of the number of births to the number living under age five, was the same in each; and that the same *invariable* law of mortality prevailed over those under the age of five years in each.

In the towns selected, the number of births registered in 1855 was 23,481. The number of persons living under age five, estimated with reference to the middle of the year and corrected for those returned at unspecified ages, was 90,260.

	Ages.	Deaths.
The deaths at different ages under five years, corrected for those returned at unspecified ages, were	0–1	3,622
	1–2	1,654
	2–3	705
	3–4	375
	4–5	252

Assuming correctness of the returns upon which the above values depend, we find the annual rate of increase of births in the *permanent* population to have been 1.1023 per cent.* We also find the number of

* Had the rate of the annual increase of the numbers living under age five (3.13 per cent.) resulted entirely from the increase of births in a permanent population, the number of births of 1855 would have been 24,457, instead of 23,481, the number registered. On the other hand, had the increase resulted wholly from migration (the annual number of births in the permanent population being constant), the number of

deaths at the different ages demanded by a constant supply of 23,481 annual births in a community influenced by migration, but subject to the above-mentioned invariable law of mortality, to be

Ages.	Deaths.
0–1	3,641.9
1–2	1,681.4
2–3	724.6
3–4	389.7
4–5	264.7

Hence of 10,000 persons born alive, there would survive, ages 1, 2, 3, 4, and 5, as follows: —

TABLE II.

Ages.	Numbers.	Logarithms.
0	10,000.0	4.0000000
1	8,449.0	3.9268053
2	7,732.9	3.8883424
3	7,424.3	3.8706555
4	7,258.3	3.8608349
5	7,145.6	3.8540387

To continue the above table, we shall need to compare the deaths and the population at the different intervals of age.

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births would have been only 22,956. The number of births registered is somewhat nearer the latter than the former of these two values.

Assuming the correctness of the returns of births, deaths, and population in the selected districts, and of the indicated rates of increase of population, it appears that 35 per cent. of the increase of the population *under age five* was due to increase of births in the permanent portion of the population, and 65 per cent. due to the movement of the migratory portion; also that 38 per cent. of the increase of population *at all ages* was due to excess of births over deaths, leaving 62 per cent. to be accounted for by excess of immigration over emigration. (A.)

TABLE III.—DEATHS, POPULATION, MORTALITY, AND LOGARITHMS OF THE PROBABILITY OF LIVING.

Massachusetts, 166 *towns*, 1855.

AGES.	REGISTERED DEATHS.	POPULATION.	MORTALITY.	LOGARITHMS, WITH THE SIGN CHANG'D, OF THE PROBABILITY OF SURVIVING EACH INTERVAL OF AGE.	
	1855.	June 1st, 1855.	Ratio of the Number of Deaths in 1855 to the Number of the Population, estimated for the middle of that year, and corrected for the Numbers at ages not specified.	Duplicate Values, each deduced from *two* consecutive Ratios in the Column of Mortality.	Arithmetrical Mean of the Duplicate Values.
Under 1 year	3,595				
1–2	1,642				
2–3	700	89,852	.0732094		
3–4	372				
4–5	250				
3–5	622		.0191772*		
5–10	595	76,566	.0077980	.0168125 .0169100	.0168613
10–15	311	71,851	.0043436	.0094179 .0094488	.0094333
15–20	672	76,854	.0087768	.0190957 .0190705	.0190831
20–30	1,817	161,544	.0112883	.0491767 .0490648	.0491208
30–40	1,388	112,489	.0123781	.0538119 .0538257	.0538188
40–50	1,035	73,604	.0141040	.0613538 .0615053	.0614295
50–60	908	45,134	.0201687	.0881465 .0884615	.0883040
60–70	942	25,766	.0366732	.1624950 .1624646	.1624798
70–80	976	12,265	.0798130	.3729434 .3595270	.3684713†
80–90	635	3,469	.1838871	1.1507632 .3622095	.9536248†
90 and upwards	129	374	.3466159		
Age not specified	119	1,472			
All ages	16,086	751,240	.0213663		
Specified ages	15,967	749,768			

* From deaths and *estimated* population, at the ages of three to five.

† The former of these values was obtained by giving *double*, and the latter by giving *triple*, *weight* to the antecedent of the respective duplicate values in the preceding column.

The method adopted in calculating the probabilities of living from the annual rate of mortality, is essentially the same as that indicated on pages 60 and 61 of the Proceedings of the American Association for 1856.

From the above Tables II. and III. the following values, from birth to age 90 inclusive, are readily deduced.

TABLE IV.—PROPORTIONS BORN ALIVE, AND SURVIVING CERTAIN AGES.

Ages.	Logarithms.	Numbers.
0	4.0000000	10,000
1	3.9268053	8,449
2	3.8883424	7,733
3	3.8706555	7,424
4	3.8608349	7,258
5	3.8540387	7,146
10	3.8371774	6,873
15	3.8277441	6,726
20	3.8086610	6,437
30	3.7595402	5,748
40	3.7057214	5,078
50	3.6442919	4,409
60	3.5559879	3,597
70	3.3935081	2,475
80	3.0250368	1,059
90	2.0714120	117.9
100	.3430527	2.20

In assigning the average number that may be expected to survive age 100 out of a stated number of births, there is room for some diversity of opinion. The influence, however, of the numbers at this extreme age upon tables of practical utility is inconsiderable.

The logarithms in Table V. were derived from those in Table IV., by the interpolation of eight additional values, namely, those at the ages of 25, 35, 45, 55, 65, 75, 85, and 95. The third differences of the logarithms from age 35 upwards in the following table constitute a constantly increasing series.

Table V. — Massachusetts Life Table, 1855.*

	The number of persons *living at certain ages, to 10,000 children born alive.* Also, the annual number of *deaths at and over certain ages, in a stationary population* supplied by 10,000 annual births.		The *years* which the (L_x) persons *living at certain ages will live*; also, the *years* which those annually *dying at and over certain ages*, in the stationary population, *have lived over those ages*; *persons living at and over certain ages*, in the stationary population.	*Years* which the (Q_x) persons *living at and over certain ages*, in the stationary population, *will live*; also, the *years* which they *have lived* over those ages.	*Average* number of *years* which those living at certain ages *will live*; also, which those *dying, at and over* certain ages, *have lived* over those ages.	*Average* number of *years* which those living *at and over certain ages will live*; also, which *they have lived* over those ages.
Ages	Logarithms.	Numbers.	$dx\int_x^\infty L_x.$	$dx\int_x^\infty Q_x = (dx)^2\int_x^\infty\int_x^\infty L_x.$	$\frac{Q_x}{L_x}$	$\frac{Y_x}{Q}$
x	λL_x	L_x	Q_x	Y_x	E_x	E'_x
0	4.000000	10,000	397,653	12,857,379	39.77	32.33
1	3.926805	8,449	388,655		46.00	
2	3.888342	7,733	380,616		49.22	
3	3.870656	7,424	373,060		50.25	
4	3.860835	7,258	365,727		50.39	
5	3.854039	7,146	358,530	10,967,776	50.17	30.59
10	3.837177	6,873	323,508	9,263,748	47.07	28.64
15	3.827744	6,726	289,508	7,731,641	43.04	26.71
20	3.808661	6,437	256,561	6,367,019	39.86	24.82
25	3.785307	6,100	225,205	5,163,297	36.92	22.93
30	3.759540	5,748	195,584	4,112,047	34.03	21.02
35	3.733016	5,408	167,699	3,204,550	31.01	19.11
40	3.705721	5,078	141,486	2,432,279	27.86	17.19
45	3.676505	4,748	116,919	1,786,956	24.62	15.28
50	3.644292	4,409	94,015	1,260,344	21.32	13.41
55	3.604475	4,022	72,919	843,806	18.13	11.57
60	3.555988	3,597	53,842	527,821	14.97	9.80
65	3.486461	3,065	37,152	301,421	12.12	8.11
70	3.393508	2,475	23,279	151,568	9.41	6.51
75	3.263163	1,833	12,471	63,578	6.80	5.10
80	3.025037	1,059	5,245.2	20,780	4.95	3.96
85	2.640633	437.1	1,599.7	5,075.5	3.66	3.17
90	2.071412	117.9	336.8	869.0	2.86	2.58
95	1.311480	20.49	46.4	94.8	2.26	2.04
100	0.343053	2.203	3.47	5.12	1.58	1.48

* This comprehensive form was first given to the Life Table by Dr. Farr, the eminent English statistician. Valuable details respecting the properties and uses of the columns Q and Y (Table V.) and Z (Table VII.) may be found in the Sixth Report of the Registrar-General (Eng.)

The integration of the functions L_x, Q_x, L'_x, and Q'_x, to obtain the values in columns Q, Y, N, and Z respectively, was chiefly effected by the brief methods detailed in the Proceedings of the American Association for 1856. The ordinary process involves a preliminary and formidable interpolation of values at annual intervals of age, and a summation of the values thus obtained. In note B is offered a modification of the method previously given, especially adapted to the computation of values at the higher ages.

A large variety of useful problems may be solved by reference to the table above. We can now only advert to some of the more obvious of its properties.

According to the law of mortality for Massachusetts, it appears, that of 10,000 children born alive, 6,437 persons *will survive* age 20;

That these 6,437 persons *will live*, in the aggregate, 256,651 years;

That the *average* number of years which they will live is 39.86;

And that the *average* number of years which they *have lived* and *will live*, that is, the complete average duration of life, past and future, in years, is 59.86, that is, 20 + 39.86.

In a *stationary population*, supplied by 10,000 annual births, there will annually occur 6,437 *deaths* of persons at and over age 20.

These 6,437 persons *dying* will have lived, in the aggregate, 256,561 years over age 20.

The *average* number of years over age 20 which they will have lived is 39.86;

Their average age at death is consequently (20 + 39.86 =) 59.86 years.

In a *stationary population* supplied by 10,000 annual births, there will be 256,561 persons constantly living at and over age 20.

This generation of 256,561 persons *will live* in the aggregate 6,367,019 years;

They *have already lived* 6,367,019 years over age 20.

The *average* number of years which they *will live* is 24.82.

The *average* number which they *have lived*, over age 20, is 24.82 years; their average age is consequently 44.82 years; and the complete *average* duration, past and future, of the generation of persons now at and over 20 years of age, or their average age at death, is (44.82 + 24.82 =) 69.64 years.

In a stationary population there constantly will be *living*, to one annual death, 39.86 persons, at and over age 20.

In a community the members of which enter in constant and uniform numbers at age 20, and retire at age 60 or before in the event of death, the average number of years that the present members *will continue* with the community is 18.18; they *have* already *been* members 18.18 years; consequently their complete average duration of membership, past and future, is 36.36 years.

According to the English Life Table (1841) these numbers would be 18.23 and 36.46, respectively.

This case approximately represents that of a community of business men, if we assume that its members enter at about the age of 20 years in nearly equal annual numbers, and retire from active life about the age of 60 years, or before in case of decease.

This table will be found of practical utility, not only for the very valuable purposes of Life Insurance, but also to the statesman and to the political economist, in the solution of many important problems, among which may be mentioned those relating to the strength and the decadence of armies in time of peace, and to the influence of immigration and emigration on the growth of populations.

"The applications and uses of National Life Tables," says Dr. Farr,* "are almost innumerable: without an intimate knowledge of their properties it is impossible to determine the laws of population, which are the basis of statistics, or to reason upon such matters without falling into great errors, of which, if it were not invidious, too many instances might be cited from current works on population and public health."

* Sixth Rep. Reg. Gen. p. 524

TABLE VI.—PREPARATORY TABLES FOR DETERMINING THE VALUES OF LIFE ANNUITIES, AND OF OTHER SINGLE LIFE BENEFITS, AT DIVERS RATES OF INTEREST FOR MONEY, ACCORDING TO THE MASSACHUSETTS LIFE TABLE.

Age.	3 per cent.		4 per cent.		5 per cent.	
	$L_x\left(\frac{1}{1\cdot03}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1\cdot04}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1\cdot05}\right)^x$	$\Sigma_x^\infty L'_x$
x	L'_x	N_x	L'_x	N_x	L'_x	N_x
0	10,000	189,437	10,000	158,231	10,000	135,595
1	8,203	179,437	8,124	148,231	8,047	125,595
2	7,289	171,234	7,150	140,107	7,014	117,548
3	6,794	163,945	6,600	132,957	6,413	110,534
4	6,449	157,151	6,204	126,357	5,971	104,121
5	6,164	150,702	5,873	120,153	5,599	98,150
10	5,115	122,031	4,643	93,311	4,220	72,992
15	4,317	98,111	3,735	72,001	3,235	53,976
20	3,564	78,061	2,938	54,969	2,426	39,491
25	2,913	61,585	2,288	41,636	1,801	28,679
30	2,368	48,151	1,772	31,277	1,330	20,671
35	1,922	37,239	1,370	23,442	980.4	14,763
40	1,557	28,388	1,058	17,069	721.4	10,410.3
45	1,256	21,228	812.8	12,291.9	528.4	7,212.3
50	1,006	15,464	620.3	8,630.9	384.4	4,875.0
55	791.5	10,876.5	465.2	5,853.5	247.8	3,184.4
60	610.6	7,291.3	342.0	3,784.4	192.6	1,984.1
65	448.8	4,570.8	239.5	2,287.9	128.6	1,156.0
70	312.5	2,609.2	158.9	1,259.6	81.33	613.51
75	199.7	1,279.4	96.75	595.36	47.20	279.41
80	99.55	490.96	45.96	219.55	21.37	99.00
85	35.44	136.02	15.59	59.44	6.911	25.73
90	8.243	25.989	3.455	10.948	1.460	4.536
95	1.236	3.363	.494	1.3177	.1988	.5210
100	.1146	.235	.0436	.0891	.0168	.0341

TABLE VI. — *continued.* PREPARATORY TABLES FOR DETERMINING THE VALUES OF LIFE ANNUITIES, AND OF OTHER SINGLE LIFE BENEFITS, AT DIVERS RATES OF INTEREST FOR MONEY, ACCORDING TO THE MASSACHUSETTS LIFE TABLE.

Age.	6 per cent.		7 per cent.		8 per cent.	
	$L_x\left(\frac{1}{1\cdot06}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1\cdot07}\right)^x$	$\Sigma_x^\infty L'_x$	$L_x\left(\frac{1}{1\cdot08}\right)^x$	$\Sigma_x^\infty L'_x$
x	L'_x	N_x	L'_x	N_x	L'_x	N_x
0	10,000	118,630	10,000	105,552	10,000	95,219
1	7,971	108,630	7,896	95,552	7,823	85,219
2	6,882	100,659	6,754	87,656	6,630	77,396
3	6,234	93,777	6,060	80,962	5,694	70,766
4	5,749	87,543	5,537	74,842	5,835	64,872
5	5,840	81,795	5,095	69,305	4,868	59,537
10	3,838	58,193	3,494	47,141	8,164	38,703
15	2,806	41,207	2,438	31,948	2,120	25,099
20	2,007	28,865	1,663	21,411	1,381	16,091
25	1,421	20,077	1,124	14,256	890.7	10,250.8
30	1,001	13,870	755.1	9,433	571.3	6,493.1
35	703.6	9,502	506.5	6,194.8	365.8	4,085.1
40	493.7	6,433	839.1	4,024.2	238.8	2,544.1
45	344.9	4,283.5	226.1	2,572.9	148.7	1,560.8
50	239.3	2,784.7	149.7	1,607.3	94.00	936.60
55	168.2	1,750.7	97.36	971.6	58.37	544.23
60	109.1	1,050.2	62.08	560.91	85.53	302.20
65	69.44	589.31	37.72	803.09	20.60	157.12
70	41.89	801.41	21.71	149.33	11.32	74.545
75	23.19	132.22	11.47	63.11	5.707	30.335
80	10.01	45.00	4.724	20.62	2.245	9.524
85	3.085	11.230	1.390	4.94	.6304	2.191
90	.6221	1.894	.2672	.7973	.1157	.338
95	.0808	.2078	.0331	.0862	.01368	.0350
100	.0065	.0132	.0025	.0051	.00100	.00200

TABLE VII. — FOR DETERMINING THE AVERAGE VALUES OF LIFE ANNUITIES, AND OF OTHER SINGLE LIFE BENEFITS, ON THE WHOLE OF A STATIONARY POPULATION, OR ON THE PART AT AND OVER CERTAIN AGES, ACCORDING TO THE MASSACHUSETTS LIFE TABLE.

Interest of Money. — Five per cent.

Ages.	$Q_x \left(\frac{1}{1{\cdot}05}\right)^x$	$\Sigma_x^\infty Q'_x$	$\frac{Z_x}{Q'_x} - 1$
	Q'_x	Z_x	a'_x ANNUITY.
0	397,653	5,682,478	13.29
1	370,148		
2	345,230		
3	322,263		
4	300,885		
5	280,918	3,946,299	13.05
10	198,606	2,717,810	12.68
15	139,258	1,851,426	12.29
20	96,695	1,246,093	11.89
25	66,504	827,263	11.44
30	45,254	540,811	10.94
35	30,402	345,934	10.38
40	20,097	216,087	9.75
45	13,013	130,868	9.06
50	8,198	76,207	8.30
55	4,982	42,192	7.47
60	2,882	21,860	6.59
65	1,558	10,360	5.65
70	765	4,332	4.66
75	321	1,510	3.71
80	105.8	417.4	2.95
85	25.3	83.2	2.29
90	4.2	12.4	1.95
95	.5	1.2	1.49
100	.03	.06	1.00

The average of the present values of one dollar, payable at the close of each year during the continuance of each of the lives of the persons now at and over the age of 20 years, *in a stationary population*, interest of money being computed at the rate of 5 per cent. per annum, is $11.89. [*Table* VII.]

EXAMPLES. — The present value of one dollar, payable at the close of each year during the continuance of the life of a person now 20 years of age, interest of money being computed at the rate of

{ 4 per cent. per annum, is $17.71
{ 5 per cent. per annum, is $15.28
[*Table* VIII.]

Our L'_x is commonly written D_x.

The N_x is that used by Dr. Farr and a few other late writers, and is equivalent to the N_{x-1} introduced by Mr. Griffith Davies, and adopted in certain standard treatises on life annuities and reversions.

Q, Q', Y, and Z retain the same signification as in the Reports of the English Registrar-General.

i is any annual rate of interest for money; as, ·03, ·04, or ·05.

Formulas for determining values of annuities, annual premiums, and single premiums, are given in the headings of the respective columns in which those values appear. [Table VIII.]

$$L'_x = L_x \left(\frac{1}{1+i}\right)^x$$

$$Q'_x = Q_x \left(\frac{1}{1+i}\right)^x.$$

N_x (which equals $\Sigma_x^\infty L'_x$) $= L'_x + L'_{x+1} + L'_{x+2} + \cdots L'_\infty$, and represents the aggregate present values of a constant sum, the $\left(\frac{1}{1+i}\right)^x$ portion of one dollar, payable at the beginning of each year, during the continuance of each of the lives of the L_x persons living *at* the age x.

Z_x (which equals $\Sigma_x^\infty Q'_x$) $= Q'_x + Q'_{x+1} + Q'_{x+2} + \cdots Q'_\infty$, and represents the aggregate present values of a constant sum, the $\left(\frac{1}{1+i}\right)^x$ portion of one dollar, payable, at the beginning of each year, during the continuance of each of the lives of the Q_x persons living in a stationary population *at and over* age x.

a'_x $\left(\text{which equals } \frac{Z_x}{Q'_x \frac{1}{(1+i)^x}} - 1\right)$ represents the average of the present values of one dollar, payable, at the beginning of each year, during the continuance of the lives of each of persons living (Q_x) in a stationary population at and over age x.

A column, represented by the well-known symbol M_x, may be constructed from values in columns L'_x and N_x by the following simple formula:

$$M_x = L'_x - \frac{i}{1+i} N_x.$$

M_x represents the aggregate present values of a constant sum, the $\frac{1}{(1+i)^x}$ portion of one dollar, payable at the end of each of the years in which the deaths of the L_x persons living at the age x will occur.

For methods for deducing from the above the values of other single life benefits, the reader is referred to the writings of Mr. David Jones, in his work on "Annuities and Reversionary Payments," of Professor De Morgan, in the Companions to the British Almanac for 1840 and 1842, and of Dr. Farr, in the Sixth and the Twelfth Reports of the Registrar-General (Eng.). These benefits may be uniform or variable, and may apply either to the entire period of life or to limited portions.

Tables, for determining the values of benefits contingent upon a combination of lives, may be framed by brief processes, in some degree analogous to those already indicated.

TABLE VIII.

Ages.	LIFE ANNUITY. — The present value, after arriving at a certain age, of one dollar, payable at the end of each year during life.		ANNUAL PREMIUM UNAUGMENTED. — The sum payable at the beginning of each year, after arriving at a certain age, which will amount to one hundred dollars at the end of the year of decease.		SINGLE PREMIUM UNAUGMENTED. — The present value, after arriving at a certain age, of one hundred dollars, payable at the end of the year of decease.	
	$a_x = \frac{N_x}{L_x} - 1.$		$\pi_x = 100\left(\frac{1}{1+a_x} - \frac{i}{1+i}\right).$		$V_x = 100\left(1 - \frac{i}{1+i}(1+A_x)\right).$	
	4 per cent.	5 per cent.	4 per cent.	5 per cent.	4 per cent.	5 per cent.
0	14.824	12.560	2.47	2.61	39.14	35.43
1	17.247	14.608	1.63	1.65	29.82	25.68
2	18.595	15.759	1.26	1.21	24.64	20.20
3	19.145	16.236	1.12	1.04	22.52	17.92
4	19.367	16.438	1.06	.97	21.67	16.96
5	19.459	16.530	1.04	.94	21.31	16.52
10	19.097	16.297	1.13	1.02	22.70	17.64
15	18.277	15.685	1.34	1.23	25.86	20.55
20	17.710	15.278	1.50	1.38	28.04	22.48
25	17.198	14.924	1.65	1.52	30.01	24.17
30	16.651	14.542	1.82	1.67	32.11	26.99
35	16.111	14.058	2.00	1.88	34.19	28.30
40	15.133	13.431	2.35	2.17	37.95	31.28
45	14.123	12.649	2.77	2.57	41.83	35.00
50	12.914	11.682	3.34	3.12	46.49	39.61
55	11.583	10.588	4.10	3.37	51.60	44.82
60	10.065	9.301	5.19	4.95	57.44	50.95
65	8.553	7.989	6.62	6.36	63.26	57.19
70	6.927	6.543	8.77	8.50	69.51	64.08
75	5.154	4.920	12.40	12.13	76.33	71.81
80	3.777	3.633	17.09	16.82	81.63	77.94
85	2.813	2.723	22.38	22.10	85.33	82.27
90	2.169	2.101	27.71	27.50	87.81	85.24
95	1.670	1.621	33.61	33.40	89.73	87.49

TABLE IX.—COMPARISON OF THE PRESENT VALUES OF A WIDOW'S RIGHT OF DOWER IN THE INCOME OF AN ESTATE WORTH $1,000, COMPUTED ACCORDING TO THE MASSACHUSETTS, THE ENGLISH, AND THE PRUSSIAN LIFE TABLES.

Ages.	Massachusetts, 166 Towns, 1855.		England, Females, 1841.	Prussia, 1839-40-41.
	5 per cent.	4 per cent.	4 per cent.	4 per cent.
25	251	231	235	226
35	237	217	216	202
45	214	191	191	170
55	180	158	155	130
65	138	118	113	92
75	88	74	74	65
85	52	43	45	45

In computing the above table, the widow's interest in the estate was supposed to continue until the moment of decease. Such tables have been sometimes framed on the assumption that the claim was to cease with the end of the year preceding that in which the death should occur.

We observe a close resemblance between the values from the Massachusetts data, and those derived from the table that expresses the law of mortality that prevails over the females of England.

The values from Prussian data are usually less than those from the English and the American observations.

We now give tables comparing the newly determined law of mortality for Massachusetts, in some of the forms in which it has been presented, with the laws which prevail over the populations of several of the communities of Europe.

The ratios of deaths to population, in Tables X. and XI. do not, in all cases, admit of direct and exact comparison, owing to want of uniformity in the intervals of age. Their relations, however, are sufficiently obvious for our present purpose. If curves be traced, to which the ratio of the number of the living to *one* annual death, at each of the intervals of age, and the age of the middle of the interval shall be coördinates, the relative vitality of the several communities at every age of life may be readily compared, and with sufficient approach to exactness.

TABLE X.—MORTALITY, PER CENT., OR, THE NUMBER OF DEATHS TO 100 PERSONS LIVING, IN DIVERS COMMUNITIES, COMPARED.

Ages.	Massachus'ts, 166 Towns, 1855.	England and Wales,* Seven Years, 1838–44.		Sweden,* Thirty Years, 1811–40.		Carlisle,† Nine Years, 1779–87.
	Persons.	Males.	Females.	Males.	Females.	Persons.
0–5	7.32	7.07	6.04	7.28	6.27	8.23
5–10	.78	.93	.90	.83	.78	1.02
10–15	.43	.50	.55	.52	.49	.50
15–20	.88	.70	.79	.54	.53	.68
20–30	1.13	.94	.94	.90	.73	.75
30–40	1.24	1.09	1.13	1.31	1.06	1.06
40–50	1.41	1.45	1.32	1.96	1.42	1.43
50–60	2.02	2.26	1.98	3.09	2.30	1.83
60–70	3.67	4.28	3.79	5.66	4.72	4.12
70–80	7.98	9.22	8.42	11.81	10.54	8.30
80–90	18.39	20.11	18.32	25.63	23.01	17.57
90 and over	34.66	36.53	34.58	42.15	39.72	28.44
All ages	2.14	2.27	2.10	2.56	2.28	2.50

* From a paper by T. R. Edmonds, Esq., published in the numbers of "The Lancet" (London) for the 9th and the 16th of March, 1850.

† Derived from values on page 418 of Mr. Milne's Treatise on "Annuities and Assurances."

TABLE XI.—MORTALITY PER CENT., OR THE NUMBER OF DEATHS TO 100 PERSONS LIVING, IN DIVERS COMMUNITIES, COMPARED.

Ages.	BELGIUM	ENGLAND AND WALES.*		SWEDEN.†	Ages.	Ages.	PRUSSIA ‡
	9 years, 1842-50. Persons.	7 years, 1838-44. Mean of Males and Females.	10 years. 1845-54. Mean of Males and Females.	20 years, 1821-40. Mean of Males and Females.			8 years, 39,40,41. Persons.
0–1	20.11	17.92		19.84	0–1		
1–2	7.19	6.55		3.80	1–3		
2–3	3.78	3.51					
3–4	2.61	2.50		1.56	3–5		
4–5	1.80	1.84					
0–5	6.99	6.54	6.85	6.43	0–5	0–5	8.02
5–10	1.09	.91	.91	.76	5–10	5–7	1.52
10–15	.72	.53	.53	.47	10–15	7–14	.78
15–20	.87	.82	.85	.59	15–25	14–20	.63
20–25	1.04					20–25	.89
25–30	1.05	.99	1.05	.97	25–35	25–30	.97
30–35	1.08					30–35	1.08
35–40	1.21	1.25	1.30	1.42	35–45	35–40	1.32
40–45	1.44					40–45	1.45
45–50	1.56	1.66	1.76	2.06	45–55	45–55	2.10
50–55	2.08						
55–60	2.75	2.95	3.04	3.57	55–65	55–60	3.57
60–65	2.77					60–65	5.58
65–70	5.38	6.22	6.43	7.61	65–75	65–75	9.09
70–75	8.41						
75–80	11.69	13.74	14.32	16.93	75–85	75–85	15.15
80–85	16.57						
85–95	22.70	28.42	29.19	32.60	85–95	85 & upw.	26.62
95 & upw.	25.79	41.46	45.22	43.64	95 & upw.		
All ages	2.42	2.19	2.28	2.34			2.70

* Ninth Rep. Reg. Gen., p. 177, and Seventeenth Rep. Reg. Gen., p. xvi.

† Eighth Rep. Reg. Gen. (Eng.), p. 276.

‡ Proceedings Am. Assoc. for the Adv. of Science, 1856, p. 56.

TABLE XII. — PROPORTIONS BORN AND SURVIVING CERTAIN AGES IN DIVERS COMMUNITIES, COMPARED.

	MASSACHU'TS, 166 towns. 1855.	ENGLAND AND WALES, 1841. Farr.	CARLISLE, 1779-87. Milne.	PRUSSIA, 1839, 40, 41. Elliott.	SWEDEN AND FINLAND, 1801-5. Milne.	BELGIUM, 1842-50. Elliott.
0	10,000	10,000	10,000	10,039	10,000	10,000
1	8,449	8,537	8,461	8,294	8,112	8,504
2	7,733	8,010	7,779	7,721	7,659	7,918
3	7,424	7,739	7,274	7,364	7,403	7,625
4	7,258	7,554	6,998	7,147	7,226	7,429
5	7,146	7,420	6,797	6,992	7,096	7,296
10	6,873	7,061	6,460	6,589	6,729	6,912
15	6,726	6,863	6,300	6,385	6,558	6,671
20	6,437	6,606	6,090	6,165	6,377	6,386
30	5,748	6,033	5,642	5,641	5,918	5,754
40	5,078	5,383	5,075	5,008	5,369	5,130
50	4,409	4,662	4,397	4,243	4,647	4,413
60	3,597	3,800	3,643	3,141	3,590	3,464
70	2,475	2,453	2,401	1,573	2,163	2,185
80	1,059	938	953	444	644	787
90	118	115	142	50	49	110
100	.2	1	9	1	0	5

TABLE XIII. — AVERAGE FUTURE DURATION OF LIFE IN CERTAIN COMMUNITIES, COMPARED.

Ages.	MASSACHUSETTS.	ENGLAND AND WALES.			SWEDEN AND FINLAND.	PRUSSIA.	CARLISLE.
	1855.	1841.		1838-44.	1801-05.	1839, 40, 41.	1779-87.
	Persons.	Males.	Females.	Males.	Persons.	Persons.	Persons.
0	39.8	40.2	42.2	40.4	39.4	36.7	38.7
5	50.2	49.6	50.4	50.2	50.0	47.1	51.3
10	47.1	47.1	47.8	47.5	47.6	44.8	48.8
15	43.0	43.4	44.1	43.6	43.8	41.2	45.0
20	39.9	39.9	40.8	40.0	40.0	37.5	41.5
25	36.9	36.5	37.5	36.6	36.3	34.0	37.9
30	34.0	33.1	34.2	33.2	32.7	30.6	34.3
35	31.0	29.8	31.0	29.8	29.1	27.1	31.0
40	27.9	26.6	27.7	26.5	25.5	23.8	27.6
45	24.6	23.3	24.4	23.1	22.1	20.4	24.5
50	21.3	20.0	21.1	19.9	18.7	17.1	21.1
55	18.1	16.7	17.6	16.7	15.6	14.0	17.6
60	15.0	13.6	14.4	13.6	12.6	11.2	14.3
65	12.1	10.9	11.5	10.9	9.9	9.0	11.8
70	9.4	8.5	9.0	8.6	7.5	7.4	9.2
75	6.8	6.6	6.9	6.6	5.7	6.0	7.0
80	5.0	4.9	5.2	5.0	4.2	4.8	5.5
85	3.7	3.7	3.8	3.7	3.2	3.8	4.1
90	2.9	2.7	2.8	2.8	2.4	3.0	3.3
95	2.3	2.0	2.1	2.1	1.7		3.5

TABLE XIV.—AVERAGE FUTURE DURATION OF LIFE OF A GENERATION, OR OF THOSE LIVING AT AND OVER CERTAIN AGES, IN A POPULATION CONSIDERED STATIONARY.

Ages.	Massachusetts.	England and Wales.*		
	1855.	1841.		1838–44.
	Persons.	Males.	Females.	Males.
0	32.3	31.9	32.5	32.0
10	28.6	28.2	28.7	28.2
20	24.8	24.2	24.8	24.2
30	21.0	20.3	20.9	20.3
40	17.2	16.4	17.0	16.4
50	13.4	12.7	13.2	12.7
60	9.8	9.2	9.6	9.2
70	6.5	6.3	6.5	6.3
80	4.0	4.0	4.1	4.0
90	2.6	2.4	2.5	2.5

* Sixth and Twelfth Reports Reg. Gen.

From inspection of Tables X. and XI. it appears (so far as the data show) that, from a point below age 5 to about age 15, *lower* rates of mortality obtain in Massachusetts than generally in European communities; that, from age 15 to divers ages between 35 and 50, the Massachusetts rates are much *higher;* after which they again *fall* somewhat *below* the European. Under the age of *five* years, mortality in Massachusetts seems more intense than in Europe generally, though less so than in Prussia, and less than was experienced in the town of Carlisle during the nine years, 1779–87, which period was before the introduction of vaccination.

In the first of the above-mentioned intervals (say from age 3 to age 15), the mortality of Massachusetts approaches more closely to that of Sweden than to those of the other European communities.

In the second of the intervals (from about age 17 to 45), it more nearly represents the mortality of Belgium, though *higher;* and from age 45 onwards, it is *lower* than, but nearer to, the average English rates, not varying greatly from the mortality of the females of England.

The mortality of Massachusetts appears to be *lower* than that of

Carlisle previous to about age 17, thence generally *higher* to about age 60, *lower* to about age 80, and *higher* from that point onward.

As a whole, the mortality of the State is better represented by that of England, than of any European country.

From about age 3 to age 35 the mortality of Sweden appears to be *lower* than that of England, and after age 35, *higher*.

In Prussia, with the exception of the intervals between the ages 15 and 25, and between ages 85 and 95, the mortality is uniformly *higher* than in England.

Through much of the interval under the age of 8 years, the mortality of Belgium closely resembles that of England; from that age to about 55 it is *higher*, thence to 85 nearly the same, and above that point, *lower*.

The Belgic rate is higher than the Prussian from age 15 to 32, beyond which point it is generally the lower.

At birth, the *average future duration of life* in Massachusetts (see Table XIII.) appears to be slightly less than in Sweden. From age 5 to age 25 inclusive, it agrees well with that of the males of England, and also with that of the population of Sweden. From age 30 onwards to advanced age, it is usually best represented by that of the females of England.

For much of the period from age 30 to age 75 inclusive, the Carlisle and the Massachusetts results do not greatly differ. Our comparisons have been made with national life tables and with the Carlisle table. The latter is introduced because of its extensive employment in this country and in Europe for insurance and in legal proceedings.

We observe, according to the Massachusetts life table, that of all born alive, somewhat less than one in six (.155) die before arriving at the age of *one* year; that one fourth (.26) die before attaining the age of *three* years; that seven tenths (.71) survive the age of *five* years; one half (.51), the age of *forty* years; one fourth (.25) the age of *seventy* years; one tenth (.11), the age of *eighty* years; and that one of every hundred born alive reaches the advanced age of *ninety* years. Great reliance cannot be reposed in conclusions respecting extreme longevity derived from the data employed in the construction of any of the tables, whether European or American, both in consequence of the less reliable character of the returns at those ages, and of their limited number.

We have seen that in Massachusetts a greater disparity exists than in European countries, between the rates of mortality at the ages of 5 to 15, and the rates from age 15 to about 45.

In the towns of Massachusetts selected, the rate of mortality at the ages of 5 to 15 was but little more than one half (.55) of the rate at the ages of 15 to 40. In England, in 1841, the rate of mortality in the former interval of age was about three fourths (.78) of the rate in the latter interval.

A similar disparity is observable on comparing the returns of deaths for the entire State for the *six* years, 1850–55, with the average of the numbers living at different ages according to *two* enumerations, — the one ordered in connection with the national census for the 1st of June, 1850, and the other in connection with the State census for the 1st of June, 1855. In the six years mentioned (1850–55), the rate of mortality in the entire State, according to the returns, at the ages of 5 to 15, was fifty-six one hundredths (.56) of the rate at the ages of 15 to 40. This ratio (.56) is *almost identical* with (.55) that of the towns selected in 1855, and strengthens the conclusion, that the feature under consideration prevails in the law of mortality of the population of the State.

The returns of deaths for the six years (1850–55) probably comprise but about *eighty-five* per cent. of the actual deaths of the period.

In the foregoing pages has been presented the Life Table for Massachusetts, with divers tables deduced therefrom. Among the more important of the latter may be enumerated: tables of average future duration of life; preparatory tables for finding the values of annuities and other single life benefits, calculated at *six* different rates of interest; and tables of life annuities, annual premiums, and single premiums at two rates of interest. Tables also have been given *comparing* the rates of mortality, the proportions living at certain ages according to the Life Table, and the average future duration of life in Massachusetts, with corresponding values in several European countries.

We defer for the present a comparison of the new results with those derived from other American observations, and with those from observations respecting select classes of lives.

We append two notes, — the former (Note A) giving the formula employed in calculating the influence of immigration and emigration on the population under the age of *five* years, preparatory to the determination of the values in the Life Table under that age; the latter (Note B) presenting the methods employed in constructing, by summary processes, from the Life Table, other tables of practical value.

NOTE A. — In a community unaffected by migration, and in which the births increase by a constant ratio, the following formula expresses the relation which holds between the number of births (L_0) in a given year, their annual ratio of increase $\left(\frac{1}{v}\right)$, the function which determines the number of deaths ($D_{0,x}$) under any age ($_x$) in the same year, according to the prevailing invariable law of mortality, supposed invariable, and the number of those living ($P_{0,5}$) under the age of five years in the middle of that year.

$$L_0 = \left\{ P_{0,5} + \int_0^5 \frac{v^{5-x} D_{0,x}}{\int_{-\frac{1}{2}}^{\frac{1}{2}} v^x} \right\} \frac{\int_{-\frac{1}{2}}^{\frac{1}{2}} v^x}{\int_0^5 v^x}.$$

Since the value of $d_x \int_{-\frac{1}{2}}^{\frac{1}{2}} v^x$ closely approximates unity, for the above formula may be substituted

$$L_0 = \frac{P_{0,5} + dx \int_0^5 v^{5-x} D_{0,x}}{dx \int_0^5 v^x}.$$

When the births are *constant*, the expression becomes

$$L_0 = \frac{P_{0,5} + dx \int_0^5 D_{0,x}}{5}.$$

This relation is more fully discussed in the Proceedings of the American Association for 1856.

$$\int_0^5 v^x \, dx = \frac{v^5 - 1}{\text{Nap. log. } v} = \frac{v^5 - 1}{\text{Com. log. } v} \times \cdot 4342945.$$

To obtain $\int_0^5 v^{5-x} D_{0,x} \, dx$, when v and $D_{0,1}$, $D_{0,2}$, $D_{0,3}$, $D_{0,4}$, and $D_{0,5}$ are given, first determine the values of $v^4 D_{0,1}$, $v^3 D_{0,2}$, $v^2 D_{0,3}$, and $v D_{0,4}$.

Then putting

S_5 for $D_{0,5} + v D_{0,4} + v^2 D_{0,3} + v^3 D_{0,2} + v^4 D_{0,1}$,
S_4 for $v D_{0,4} + v^2 D_{0,3} + v^3 D_{0,2} + v^4 D_{0,1}$,
S_3 for $v^2 D_{0,3} + v^3 D_{0,2} + v^4 D_{0,1}$,

and assuming an algebraic law of relation to connect the values S_2, S_4, and S_8, we have

$$\int_0^5 v^{5-x}\ D_{0/x}\ dx = \frac{S_5 + S_4}{2} - \frac{D_{0/5} - v\ D_{0/8}}{24}.$$

NOTE B.—ON METHODS EMPLOYED IN THE CONSTRUCTION OF CERTAIN TABLES.

$$Q_x = dx \int_x^\infty L_x.$$

$$Y_x = dx \int_x^\infty Q_x.$$

$$N_x = \Sigma_x^\infty L'_x = \Sigma_x^\infty \frac{L_x}{(1+i)^x},$$

$$Z_x = \Sigma_x^\infty Q'_x = \Sigma_x^\infty \frac{Q_x}{(1+i)^x}.$$

On account of the obvious similarity of construction of Q and Y, and also of N and Z, we need only present the methods adopted in deducing Q from L, and N from L'.

From age five onwards to advanced age, the values of L and L' are given quinquennially; from birth to age five, annually. The construction of the values in columns Q and Y, at ages earlier than five years, differs. Let S_x and S'_x represent the sum of the values of L_x and L'_x respectively, at and over any age x, at equidistant intervals of n years; that is, let

$$S_x = L_x + L_{x+n} + L_{x+2n} + \cdots$$

and

$$S'_x = L'_x + L'_{x+n} + L'_{x+2n} + \cdots$$

L_x and L'_x are general terms of series of *positive* values, that *vanish* when x is taken sufficiently great.

We remark, that

$$dx \int_x^\infty L_x = dx \int_x^{x+n} S_x,$$

and

$$\Sigma_x^\infty L'_x - \frac{L'_x}{2} = \Sigma_x^\infty S'_x - \frac{S'_x}{2}.$$

We then assume the following formulas of integration,

$$Q_x \left(\text{or } dx \int_x^\infty L_x, \text{ which equals } dx \int_x^{x+n} S_x\right),$$

equals

(A) $$n\left(\frac{S_x+S_{x+n}}{2}-\tfrac{1}{12}\cdot\frac{m\,D_x+D_{x-n}}{m+1}\right);$$

and

$$N_x\left(\text{or } \Sigma_x^{\infty} L'_x, \text{ which equals } \Sigma_x^{x+n} S'_x-\frac{S'_x}{2}+\frac{L'_x}{2}\right),$$

equals

(B) $$n\left(\frac{S'_x+S'_{x+n}}{2}+\frac{L'_x}{2}-\frac{n^2-1}{12\,n}\cdot\frac{m\,D'_x+D'_{x-n}}{m+1}\right),$$

in which

$$D_x=L_x-L_{x+n},$$

and

$$D'_x=L'_x-L'_{x+n};$$

Let m equal *unity*, when the ratio, $\frac{D_{x-n}}{D'_x}$ or $\frac{D'_{x-n}}{D'_x}$, is *greater* than $\frac{4}{9}$, and *less* than $2\frac{1}{4}$.

Let m equal	2,	when the ratio is between	$2\frac{1}{4}$ and $3\frac{1}{2}$
	3,		$3\frac{1}{2}$ and $5\frac{1}{2}$
	4,		$5\frac{1}{2}$ and $7\frac{1}{2}$
	5,		$7\frac{1}{2}$ and $9\frac{1}{2}$
	$5\frac{1}{2}$	*tenths of the ratio*, when the ratio is between	$9\frac{1}{2}$ and 16
	5		16 and 40
	4		40 and 250
	3 *tenths of the ratio*, when the ratio exceeds 250.		

These ratios, except at quite advanced ages, will commonly be such that m will equal *unity*, and the values of Q and N will not then differ from those that result from the assumption of an *algebraic* law of relation connecting the *four* values of S_x or S'_x, at the ages $x-n$, x, $x+n$, and $x+2n$.

If in (A) for S_x+S_{x+n} the sum of the values of S_x at the limiting ages x, and $x+n$, we put G, and for $\frac{m\,D_x+D_{x-n}}{m+1}$ we put H; and in (B), in like manner, put G' and H', we shall have

(C) $$Q_x=n\left(\frac{G}{2}-\frac{H}{12}\right),$$

and

(D) $$N_x=\frac{n\,G'}{2}+\frac{L'_x}{2}-\frac{n^2-1}{12\,n}\,H'.$$

When it is desired to determine the values of Q or N from but *three* given equidistant values of S, or S', for H or H' we put the second difference of the three values; this is equivalent to assuming that the three values are connected by an *algebraic* law of relation.

If in (C) we let H be *zero*, Q_x becomes merely the product of the average $\left(\frac{G}{2}\right)$ of the values of S_x at the limiting ages x, and $x+n$, by (n) the number of years in the interval; and is equivalent to assuming that a law of arithmetical progression connects the values of S_x within the limits.* When the interval of age is quinquennial, $\frac{n^2-1}{12\,n}$ equals $\frac{4}{10}$.

The operations in (C) and (D) may receive verbal interpretations.

To obtain Q_x; from the average of the limiting values $\left(\frac{S_x+S_{x+n}}{2}\right)$ of S_x, subtract one twelfth ($\frac{1}{12}$) of *a mean* (H) of the second differences (D_{x-n}, and D_x) of the four consecutive values (S_{x-n}, S_x, S_{x+n}, and S_{x+2n}) of S_x, one of which (S_{x-n}) shall precede, and another (S_{x+2n}) follow the values at the limiting ages (x and $x-n$), and multiply by the number of years (n) in the interval of age.

To obtain N_x; multiply the average of the limiting terms of S'_x by the number of years (n) in the interval of age, add one half of the value of L', corresponding to the age, and subtract $\frac{n^2-1}{n}$ twelfths of *a mean* (H') of the second differences (D'_{x-n} and D'_x) of the four values of S'_x at the ages $x-n$, x, $x+n$, and $x+2n$.

We remark that H and H' are *arithmetical* means *only* when m equals *unity;* in other cases the greater weight is commonly given to the less of the second differences.

By giving to m the values which we have mentioned above, we are enabled readily, and without resort to logarithmic tables, to arrive at values that closely approximate those that would have resulted from the integration of the exponential function

$$a+bx+c\,d^x$$

which may be assumed to equal S_x or S'_x. a, b, c, and d are

* This very simple form does not differ essentially from that given by Dr. Farr in the Fifth Report of the Reg. Gen. (Eng.), and is sufficiently accurate for the earlier ages, if the uniform interval of age (n) is not larger than quinquennial.

constants to be determined from the *four* values of the functions (S_x or S'_x) corresponding to the specified ages $x - n$, x, $x + n$, and $x + 2n$.

d will, in all cases, be *positive*, and the curve represented by above exponential function, if referred to rectangular coördinates, will have *no point of contrary flexure.*

If S_x or S'_x be represented by the algebraic function

$$a + bx + cx^2 + dx^3,$$

the curve, to which the above is the equation, if referred to rectangular coördinates, will have *a point of contrary flexure* within the limits of the ages $x - n$, and $x + 2n$, whenever the ratios of the second differences $\left(\frac{D_{x-n}}{D_x}\right)$ of the values of S_x corresponding to the ages $x - n$, x, $x + n$, and $x + 2n$ is *greater* than 2, or *less* than $\frac{1}{2}$; and the larger the ratio, if greater than 2, or the smaller the ratio, if less than $\frac{1}{2}$, the more eccentric the curve.

If in (A) we give to m the value

$$\frac{(r - 1 - 12\,\delta)\,r}{12\,\delta\,r - r + 1},$$

in which

$$r = \frac{D_{x-n}}{D_x},$$

and

$$\delta = \frac{1}{2}\,\frac{r+1}{r-1} - \frac{1}{\text{Nap.log.}\,r},$$

we shall obtain for Q_x precisely the values that would have resulted from the direct integration within the limits x and $x + n$, of the exponential expression,

$$S_x = a + b\,x + c\,d^x.$$

Above age 5, the values of Q_x were formed by successively adding to the previously determined value of Q_5, the values of the definite integrals of L_x for the ages 4 to 5, 3 to 4, 2 to 3, and 1 to 2, determined according to algebraic laws of relation, involving, in the first case (that from 4 to 5), *three*, and in the other cases *four* of the given equidistant values of L_x. The integral from *birth* to age 1 was determined by assuming that the values at ages 0, 1, and 2 were connected by the parabolic law of relation,

$$L_x = L_0 - (L_0 - L_1)\,x^b,$$

in which b obviously equals

$$\frac{\log.\,(L_0 - L_2) - \log.\,(L_0 - L_1)}{\log.\,2}.$$

The value of $\int_0^x L_x\,dx$, the required integral, is

$$x\left(L_0 - \frac{L_0 - L_1}{1 + b}\right).$$

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5. On a New Form of Arithmetical Complements. By Thomas Hill, of Waltham, Mass.

If we give the name of arithmetical supplement to the arithmetical complement diminished by one, or, in other words, to the complement obtained by subtracting each digit of a number, zeros included, from the highest digit of the system; (that is, in decimal notation from nine) then the following theorem is manifestly true.

If from the supplement of any whole number we subtract the same number that we add to the whole number, the sum and difference thus obtained are supplements of each other.

Thus 1863 + 857 = 2720 and 8136 — 857 = 7279; and 1863 is the supplement of 8136, and 2720 of 7279. These supplements may be used in arithmetical machines by printing the supplement of each digit in a smaller type by its side, so that we add by looking at the larger figures, and subtract by looking at the smaller. Thus the example already given may be printed

$$1_8 8_1 6_3 3_6 \pm 857 = 2_7 7_2 2_7 0_9.$$

Thinking that possibly other uses might be found for them, I have thus called the attention of computers to them.

II. ASTRONOMY.

1. ZODIACAL LIGHT.* By CHARLES WILKES, U. S. N., Washington, D. C.

OBSERVATIONS on the phenomenon of the Zodiacal Light were assigned as part of the duties of the Exploring Expedition, and claimed particular attention during the continuance of the voyage. In order to insure that no opportunity should be lost, and that every advantage might be taken of the opportunities when they occurred, they were made a part of the duties of the officers of the watch, with instructions when this phenomenon was visible to report it to the commander, to observe the altitude of its apex, subtension of the base, note its central line among the stars and its boundary as defined by them; the phases of light which it exhibited, as well as the variations which it underwent, with the hour and minute of the observation; and lastly, to draw a diagram of its appearance.

The number of observations taken by others and myself in the squadron were about one hundred and fifty reliable ones. For some of these I am indebted to members of the Scientific Corps, and particularly to Professor James D. Dana, the Geologist of the Expedition.

The first time we passed through the tropics, our observations were few, the weather being unfavorable, and the sky, for the greater part of the time, obscured. The phenomenon, though often seen, was illy defined, and afforded no well-marked outline of its extent or appearance. Its central line was seen to correspond with the ecliptic, and it was observed to change its phase and azimuth on each successive ob-

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* This paper on the Zodiacal Light has been prepared for several years as a part of the results of the Exploring Expedition to the South Seas. Its publication has been delayed until the volume of which it forms a part was printed by the government. The delay has placed within my reach many observations which have been made since, all of which have tended to confirm me in the belief that the explanation I offer for this phenomenon, which has so long interested astronomers, is the true and only one which will meet all the facts derived from observations.

servation as we passed rapidly to the southward. Afterwards, on our various routes during the voyage through the low latitudes, we were more fortunate, and very many opportunities were offered of making observations, which we endeavored to improve. Nevertheless, I regretted the loss of the first opportunities. Had the observations been continuous, I should be enabled to compare advantageously its extent, outline, and phases with the observations as we returned in an opposite direction; notwithstanding, I feel satisfied that I have obtained sufficient data to throw much light on this phenomenon — enough, I trust, to disprove the theories heretofore advanced respecting it, and to explain satisfactorily its cause.

All the observations made on the Zodiacal Light, since it first attracted attention (nearly two centuries ago), prove that its phases and general appearance have not changed.

The theories which have been entertained of the Zodiacal Light have been various. Some derive it from the atmosphere of the sun; that it is illuminated matter thrown off from his equator, revolving with immense velocity, which takes a lenticular shape; others, that it is a nebulous ring, with the sun for its centre, extending near to or beyond the earth's orbit; and again, another hypothesis supposes that this nebulous ring has the earth for its centre; and others again surmise that this phenomenon is nebulous matter floating in space, to which the periodical showers of stars may be traced, as the earth happens in its orbit to pass through or encounter them. It seems impossible to reconcile any of these hypotheses or surmises with the facts which close observation has developed.

The theories referred to fail to satisfy us of their correctness, as they are soon perceived to be inconsistent with the facts. It would be unnecessary here to enter into any discussion of the subject of either of them; that which has been of late offered by one who has been so persevering and continuous an observer, has been so effectually disproved by the able pen of the President of the University of Alabama, in his discussion of the hypothesis * advanced, that I need add nothing further. I have carefully examined these observations; and I lend a hearty acknowledgment to the industry and constancy which the observer (Rev. Mr. Jones) has evinced, but I can see nothing which

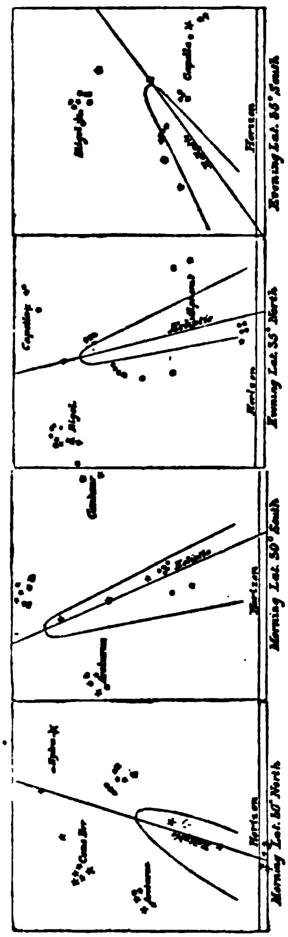
* See Journal of Science and Arts, Second Series, Vol. XXI., March, 1856.

will sanction the adoption of his theory. Although he set out to avoid any bias toward old theories, he seems tenaciously to have clung to the ring theory. I feel much gratified to have so many proofs as his observations offer of the accuracy of those made on the Expedition. I have used a different projection of the phenomenon, and I think Mr. Jones may have been led into a misconception of it by projecting the Zodiacal Light always on the ecliptic, which does not present the phenomenon naturally to the eye, or as seen; if he had projected it on the great circles, passing through the vertical and the horizon, many of the appearances which seem to have puzzled him would have been easily accounted for, and must have led him to a different result.

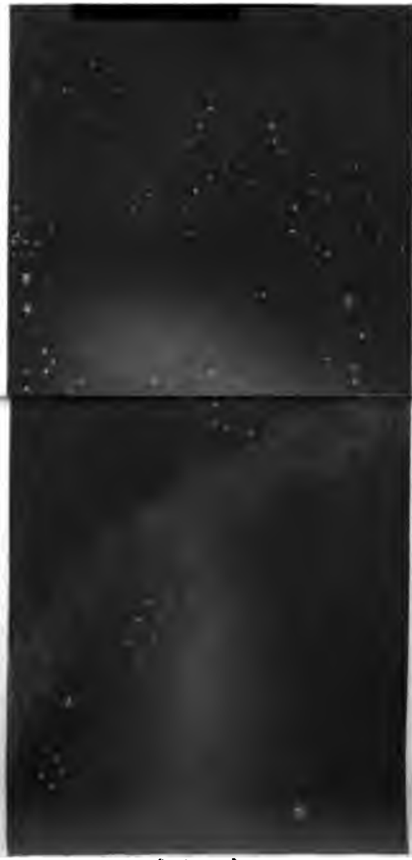
I will endeavor to describe and illustrate this phenomenon, as derived from our observations, as well as those of others.

The Zodiacal Light, when first visible on a clear horizon, appears as a semicircular arc, with 6° to 10° base, well defined and distinct to the eye, though probably no two persons would trace the same outline of it, unless accustomed to make the observation. As darkness progresses, this semicircular arc elongates upwards in successive altitudes, sometimes to the altitude of 60°. When it has attained its highest point, the diffused light becomes visible, extending on each side over a large area, until lost in the obscurity of night. It is totally different from the extended light of sunset, or the diffused light of twilight. When it becomes visible, it continues so, gradually lessening in height, until the whole is lost beneath the horizon. Its apex is always observed in the ecliptic, to the east or west of the sun, usually at the distance of 60° to 80°, but at times, under favorable circumstances, it is seen to extend as far as 110°.

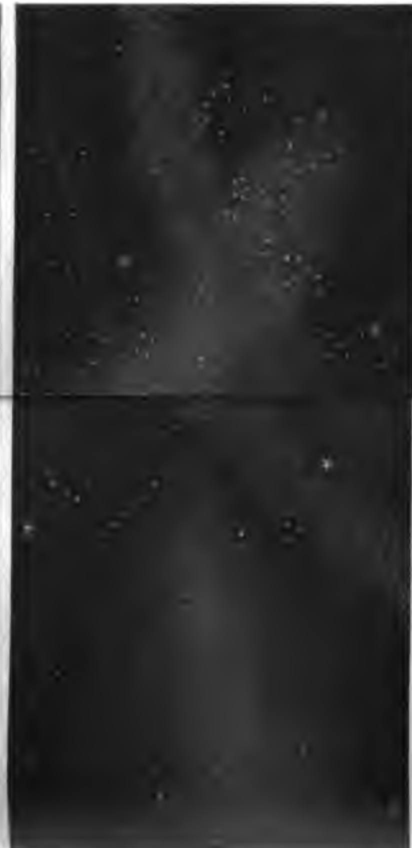
The evening and morning Zodiacal Light in the same latitude do not correspond in phase or azimuth. This, though among the remarkable facts it exhibits, has never yet been taken into consideration in any of the theories hitherto offered. Its peculiar phase and the constant change of azimuth and inclination, whether the observer remains stationary for any time, or varies his position in latitude, should satisfy every one that it cannot be far removed from the earth. When an observer is stationary, these changes take place very slowly, and it may be said to alter its phase very little during a lunation, or whilst it is visible in high latitudes; but a longer period, more attentive and closer observation, will satisfy every observer that its variations in azimuth correspond to the angular changes of the plane of the ecliptic with the observer's horizon, or with the vertical circle passing through his zenith. Its phases will be seen to be dependent upon the latitude. The vividness of the light, and its extent, are in like manner to be ascribed to the observer's position on the earth. Within the tropics, and when the ecliptic is perpendicular to the horizon, the Zodiacal Light is confined to a slender column, having its diffused light widely extended. Without the tropics, it is always seen very much inclined to the horizon. It then assumes the appearance of a cone, cut more or less obliquely by the horizon. It attains its greatest altitude in the first of these positions, when it not unfrequently may be traced to the zenith. In order to make these remarks fully evident to the reader, the annexed diagrams show the appearance of the morning and evening Zodiacal Light, on the same day, in corresponding north and south latitudes, though not on the same meridian. It will be seen that the cones are more or less inclined, and in opposite directions; this is owing to the observations being made under a different angle with the ecliptic. Corresponding observations on the same day, equally removed from the equator, and on the same meridian, could not be obtained. These diagrams prove most conclusively that it must be the same object, seen from positions to the north and south of the ecliptic, and that the phenomenon must have its locality within the tropics. They mark the characters which have been so frequently noticed, but have failed to claim attention in the formation of theories.



The accompanying plate shows true representations of the morning and evening Zodiacal Light, within the tropics, and in north and south



23° S. Morning



35° N. Evening

48° N. Morning

latitudes, though under different meridians, with the diffused light, which is at times of greater extent than at others.

The Zodiacal Light is not perceptible till the twilight ceases. This, however, gives a very indefinite idea of the time when it is first seen after sunset, to those situated on different parts of the earth's surface. To an observer within the tropics, there is but a short twilight, and darkness follows soon after the sun sets or dips below the horizon, indicating that there is little reflected light within the tropics, to produce twilight; or, otherwise expressed, the rays of light do not fall sufficiently oblique on the atmosphere to produce twilight. This is more evident when the observer's situation corresponds with the plane of the ecliptic; there night ensues immediately, and the sun's rays appear to be cut off, and not reflected. Although the Zodiacal Light has a higher elevation in the latitudes beyond the tropics, when the sun attains his greatest declination, yet, owing to the long duration of twilight, it is not so visible or distinct as when the twilight is shorter, or about the time of the vernal and autumnal equinox.

The morning Zodiacal Light does not resemble that of the evening either in color, phase, or inclination. Its color is of a cold, silvery hue, instead of the warm golden or purplish tint of the evening. It often shows in the morning, within the tropics, as a bright brush of light — like a ray of the aurora, though without its vacillating or transitory pulsations. The brush usually appears about two hours and a half before daylight or sunrise. I have seen it reach the zenith, its width being only one and a half or two degrees; it then spreads rapidly, declines in height, until its altitude is between 50° and 60°, when the diffused light extends along the horizon more than thirty degrees, and over the sky above. An hour and a half before the sun rises, the brush of light sometimes changes its phase rapidly; then the phenomenon becomes extremely beautiful, as if a gossamer veil had been suddenly unfolded from it and stretched across the sky, until it vanishes in thin air, and through which the stars of the smaller magnitudes are seen to twinkle quite distinctly.

Thus it will be seen that there is a great difference in the appearance of the Zodiacal Light, under the effect of a retreating or approaching sun. The tone of the light may easily be accounted for in the cool, grayish tints of the morning atmosphere, while that of the evening preserves the warmth of the closing day.

The Zodiacal Light can never be mistaken for either dawn or twilight; the latter is the effect of reflected light, more diffused, constantly changing, and never well defined. I have never observed any crepuscular rays to accompany the morning or evening Zodiacal Light, nor any reflection of the light on the opposite sky. It stands alone and distinct. Its central line is parallel to the ecliptic, a little to the north or south of it, but more frequently it coincides with it.

I have now given a general description of this phenomenon from careful observations made upon it. In order that the facts derived may be clearly understood, I shall repeat them in a condensed form, before entering into an explanation of the phenomenon, derived from our observations and my investigation. The reader is referred to the diagrams and plates for the appearance which the Zodiacal Light exhibits, when seen from different situations on the earth.

1. The Zodiacal Light occupies a constant relative position in the plane of the ecliptic, preceding or following the sun.

2. Its central line is parallel with or coincides with the ecliptic.

3. Its apex varies in distance, 60° to 110°, from the sun. Its height above the horizon seldom exceeds 60°.

4. Its azimuth changes with the sun and with the observer's position on the earth.

5. Its inclination alters with the position of the observer in latitude, from the vertical down to an acute angle with the horizon.

6. The morning and evening Zodiacal Light are different in phase, color, altitude, and inclination, depending upon the angle subtended between the observer's horizon and the plane of the ecliptic.

7. Its apex lies always *south* of the zenith when the observer is north of the ecliptic, and *north* of the zenith when he is to the south of the ecliptic.

8. When the ecliptic passes through the zenith of the observer, the column of light is vertical to the horizon; it then assumes the appearance of a narrow belt, with a well-defined apex.

9. North or south of the ecliptic, the Zodiacal Light exhibits a broader phase, but less in altitude than when under it.

10. The Zodiacal Light is never seen until the sun has set, and twilight ended, or until all reflected light is cut off; therefore, its visibility in high latitudes depends upon the continuance of twilight.

11. Owing to the length of twilight, the Zodiacal Light is seldom

seen near the limiting parallel. The limiting parallels vary with the sun's declination.

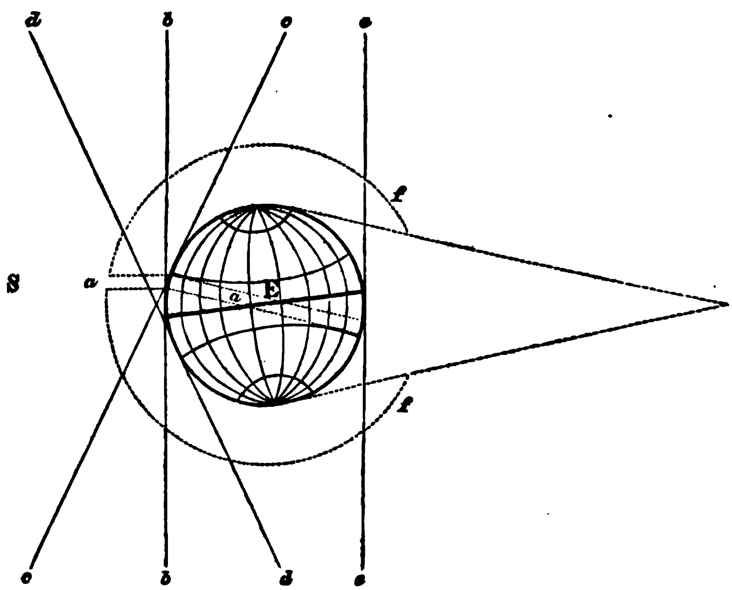
12. The sun's rays falling perpendicularly on the atmosphere within the tropics are not reflected; consequently, after sunset, there is little or no twilight.

These facts go to prove that this phenomenon is the result of the illumination of that portion or section of the earth's atmosphere on which the rays of the sun fall perpendicularly.

It will readily be conceived that rays of light will illuminate a column or portion of atmosphere on which they may fall, when no reflected or diffused light interferes to prevent its being visible. As a well-known illustration, I would cite, that if the direct rays of the sun are admitted through a hole in a shutter into a darkened room, the atmosphere, and the particles floating in it, become as visible and distinct, when all reflected light is cut off, as any well-defined object. It is this which we believe takes place, when the rays of the sun fall perpendicularly on our atmosphere and produce such an effect, which becomes visible on the earth's surface when this column is above the horizon, within the limiting parallels, and after the twilight has ceased.

The whole earth is constantly exposed to and revolves in the sun's rays. A part of these rays only fall perpendicularly on our atmosphere, while all others strike it obliquely, are reflected and refracted by it. As the earth revolves, this column or section of the atmosphere which lies in the ecliptic or earth's orbit becomes illuminated in succession, as marked on the diagram, and thus appears permanently attendant on the sun, either preceding or following him in the ecliptic, and it is this section or cone which is visible when all reflected light is cut off, or darkness reigns, and has been named the Zodiacal Light.

This theory seems to account for all the phenomena which the Zodiacal Light exhibits. In order better to explain our meaning, we refer to the following diagram to illustrate it, wherein *S* represents the sun's rays; *E*, the earth and its atmosphere; *a*, the rays which fall perpendicularly on the atmosphere in the plane of the ecliptic; and *a*, the zone on which they fall as the earth revolves; *b b′*, *c c′*, *d d′*, and *e e′*, the rational horizons of observers at different positions on the earth's surface. It is evident that the illuminated rays will be vertical to an observer when the horizon is *b b′*. To an observer whose horizon is *c c′*, these rays will be a section which is cut obliquely by the horizon, and it will appear to be more inclined the further that observer's distance is removed from the equator, or his latitude increases, until it



becomes invisible at the limiting parallel. To an observer whose horizon is at *d d′*, this column of illuminated rays will appear the same, though inclined in the opposite direction; and it will be lost sight of when the limiting parallel on that side is reached. To an observer whose horizon is *e e′*, it is impossible that any part of the column of illuminated rays forming the Zodiacal Light can be seen, but it will readily be perceived that a faint illumination may take place of a small portion of the atmosphere lying beyond the earth's shadow, at *f f′*, so as to produce the "glimmer of eastern light," called by the German writers "Gegenschien," which, it is evident by the diagram, cannot in any way be connected with or caused by the Zodiacal Light.

To observers whose horizons are represented in the diagram, the morning and evening Zodiacal Light must have a different phase and inclination; the observer's position with the ecliptic being changed materially by the rotation of the earth, so as to alter the angle between that circle and the horizon, and this will always be the case, unless he is situated on the part of the earth's surface where the days and nights are equal, or where the angle between the ecliptic and horizon remains the same to the observer after the earth has made a half revolution. When an observer is situated immediately within the perpendicular rays, or on the plane of the ecliptic, and whose horizon is at $b\ b'$, the phenomenon will appear as a belt, or narrow strip, or column of light, and will apparently be seen to attain a higher altitude. But to those with the horizon at $c\ c'$, $d\ d'$, north or south, these illuminated rays will no longer appear as a belt, but take the form of a cone, produced from the rarefied or upper atmosphere being less susceptible of illumination than the lower, and by the effect of a column seen in perspective; and as the angle between the ecliptic and horizon changes, the cone must become more and more oblique. This, our observations have satisfied us, is the case.

We know the azimuthal angle of this belt or cone changes with the declination of the sun or the observer's position, and is so great as to satisfy us that the phenomenon cannot be far removed from our earth, but must be closely connected with the atmosphere.

As this column or cone of illuminated atmosphere has apparently a visible extent in the heavens, along or parallel with the ecliptic, both preceding and following the sun, it has, naturally enough, produced the delusion that it belongs to, or is connected with, the heavens; but it will readily be seen, after a moment's reflection, how this illusion may take place, and will satisfy us that this phenomenon, as seen projected in the heavens, with the starry host twinkling through it, may produce the same effect; and we are only able to overcome this ocular deception by proving, as we have done by attentive and close observation, that it is of this earth, or closely connected with it, from its constantly undergoing great changes in azimuth, phase, inclination, and altitude, as well to a stationary observer as to one who is passing over the earth's surface from north to south, and *vice versa*. If it were distant, or had its origin in the heavens, this would not be the case; these rapid changes could not then take place: therefore, we are compelled to admit that we are deceived, and that its locality must be in the atmosphere surrounding this globe, and be an illuminated section of it which becomes visible to us as soon as twilight ceases or darkness ensues. That it lies between us and the Milky Way is evident, for when bright it nearly eclipses that starry nebulæ. Indeed, the whole phenomena accompanying the Zodiacal Light are fully accounted for by this simple theory, and strictly in accordance with all the observations made upon it, up to the present time.

Now it may be understood how all bodies giving sufficient light to illuminate the atmosphere by perpendicular rays may produce an effect similar to the Zodiacal Light, though in a much less degree. I have seen a corresponding appearance to accompany the moon and the larger planets, after they have set.

The phases of the Zodiacal Light are generally more visible in the absence of the moon, but I have several times observed them before dawn, when they were too bright to be eclipsed by that luminary. As a general rule, the observations are limited to the first and last quarters, those of the morning being visible in the former, and those of the evening in the latter.

It has been remarked above, that it is impossible to see the Zodiacal Light at midnight, the observer's horizon being $e\ e'$, but there are rays of light passing over and under the observer's position, which afford a feeble illumination to the atmosphere beyond the earth's shadow, at $f\ f'$, which may be seen. From this effect, we frequently experience nights less dark; and there are positions in the earth's orbit where this result will be greater and more apparent; and, as remarked before, may have produced those appearances to which the German writers have repeatedly called attention, and led the author of the Japan Expedition to the belief, that they were produced by the morning and evening Zodiacal Lights, visible at the same moment.

III. PHYSICS AND CHEMISTRY.

1. Preliminary Researches on the Alleged Influence of Solar Light on the Process of Combustion. By Prof. John Leconte, M. D., of Columbia, South Carolina.

A popular opinion has long prevailed in England, and, perhaps, in other countries, that the admission of the light of the sun to an ordinary fire tends to *retard* the process of combustion. In some instances, the practice of placing screens before the fireplace, or of closing the shutters of the apartment, may be traced to the prevalent belief, that the access of sunlight to the burning materials is unfavorable to the continuance of the phenomenon of combustion. Most physical philosophers, very naturally, regard this opinion as a mere *popular prejudice;* probably originating in the well-known apparent dulling or obscuration of flames and of solid bodies in a state of ignition, which takes place when they are exposed to strong light. The flame of a jet of burning hydrogen is scarcely visible in the diffused light of a clear day; that of an ordinary alcohol lamp is barely appreciable to the eye when exposed to the direct sunshine; while a portion of ignited charcoal, which glows in the dark, appears to be extinguished when placed in the sunlight. These familiar phenomena, attributable to well established physico-physiological laws, seem to afford a much more rational explanation of the origin of the popular opinion, than to suppose it to be based upon accurate observations relating to the actual rapidity of burning. About 32 years ago, Dr. Thomas M'Keever published a series of experiments in the "Annals of Philosophy,"* which seemed to show that there is a real foundation for the popular impression, and that solar light *does* actually retard the process of combustion. So far as I am aware, these remarkable experiments have never been repeated. Leopold Gmelin, in his "Hand-Book of Chemistry," † announces Dr. M'Keever's results without comment.

* Annals of Philosophy, New Series, vol. 10, p. 344. Nov. 1825.

† Leopold Gmelin's Hand-Book of Chemistry (Cavendish Society's Translation), vol. 2, p. 35. London, 1849. A contemporary journal, in noticing these results,

The important bearing which they appear to have on the influence of solar light on chemical processes, as well as on the modern dynamical theory of the mutual convertibility of the so-called imponderables, induced me, during the months of May and June last, to undertake a series of experiments, with the view of testing the validity of Dr. M'Keever's conclusions. The subjoined table will exhibit his results in a convenient form for future use.

Expt.	Green Wax Taper lost in 5 minutes.		Ratio.
1	In dark, temp. 67° F. 9.25 grains.	In sunshine, temp. 78° F. 8.5 grains.	1:1.088
	Taper lost, by burning 7 minutes in		
2	Dark, temp. 67° F. 11 grains.	Sunshine, temp. 78° F. 10 grains.	1:1.100
	Mould Candle, to consume 1 inch, took, in		
3	Dark, temp. 68° F. $56^m\ 0^s$	Sunshine, temp. 80° F. $59^m\ 0^s$	1:1.053
	Taper, to consume 1 inch, took, in		
4	Dark, temp. 67° F. $4^m\ 30^s$	Sunshine, temp. 79° F. $5^m\ 0^s$	1:1.111
	Taper, in sunshine, lost, in 10 minutes,		
5	In painted lantern, 16.5 grains.	In uncoated lantern, 15 grains.	1:1.100

A *sixth* experiment of a similar character, made in strong *moon-light*, indicated no such diminution in the rate of consumption. The conclusion to which Dr. M'Keever came, was, that solar light *does exercise* a positive retarding influence on the process of combustion.

remarks : "It has always been considered a vulgar error, that the sun's light extinguishes a fire; but the following experiments by Dr. McKeever put the matter beyond a doubt." (Brewster's Edin. Journal of Science, vol. 5, p. 180.—1826.)

He supposes this effect to be owing to the well-known influence of the solar rays on many chemical processes; in some instances accelerating them, but in others, retarding them. Under this point of view, the *chemical* rays may be supposed to exercise a *deoxidizing* power, which, to some extent, interferes with the rapid oxidation of the combustible matter. In confirmation of this opinion, Dr. M'Keever made an experiment, which appears to indicate, that a taper burns more rapidly in the *red* than in the *violet* extremity of the solar spectrum.

In attempting a repetition of Dr. M'Keever's experiments, I found it impossible to secure that freedom from agitation in the atmosphere, during the exposure of the burning body to the influence of sunshine in the *open air*, which such an investigation demanded. This was his method of conducting the *first four* experiments given in our table. The powerful influence exercised by comparatively slight disturbances in the air, on the rapidity of combustion, renders attention to this circumstance of controlling importance. His method of obviating this difficulty by the use of lanterns (as indicated by experiment 5) is objectionable, from the impossibility of securing precisely identical conditions in relation to the supply of air in the interior. There were, likewise, other considerations which urged me to modify his method of conducting the investigation. It occurred to me, that, as in his experiments the *temperature* of the air which supplied oxygen for combustion in the sunshine was about 12° Fahr. above that in the darkened room, the rarefaction produced by heat might exercise some influence in retarding the rate of burning in the sunlight.

In conducting my experiments, I endeavored to secure *two conditions;* namely,

1. Absolute *calmness* in the atmosphere.

2. Exposure of the flame to the influence of intense solar light, *without heating the surrounding air.*

The *first* condition was secured by performing *all* of the experiments in a large lecture-room, with all the doors and windows closed. To secure the *second* condition, I employed a portion of the apparatus belonging to a large solar microscope, consisting of the reflecting mirror, the condensing lens and tube, together with the mechanical arrangements for adjusting the direction of the light. As the condensing lens was upwards of four inches in diameter, I hoped to *exaggerate* enormously whatever effect the light might exert, by concentrating it on a comparatively small area. Inasmuch as the aperture in the window through which the light was admitted was completely closed by this arrangement, the exterior agitations of the atmosphere were not felt in the room; while the pencil of light, thus thrown on the flame, traversed it, as well as the surrounding air, *without imparting a sensible amount of heat to the latter.*

I used the best *wax-candles* (as they are called in the shops), *four* to the pound, costing about fifteen cents apiece.* By allowing them to burn a sufficient length of time to form a well-defined cup for the melted wax, and carefully turning the wicks, so as to render them self-snuffing, the combustion was found to go on with remarkable uniformity in a calm atmosphere. The rate of burning was determined in the following manner: A portion of candle, three or four inches in length, was secured to the bottom of one of the scale-pans of a tall balance, and ignited; after allowing it to burn for ten or fifteen minutes, so as to secure a steady flame of constant size, it was *nearly* balanced by adding weights to the opposite scale-pan, allowing a slight preponderance to the candle-pan. In a short time the equilibrium was established by the burning of the candle; the precise *time* at which the balance indicated a condition of equilibrium was *accurately noted.* Next, *a given* weight (say sixty or one hundred grains), was withdrawn from the weight-pan, and the *time* of restoring the equilibrium by the loss of weight in the burning candle was, in like manner, recorded. In this manner, the rate of combustion was determined by observing the *time* occupied in consuming a *given weight* of the burning matter. The arrangements described above enabled me to perform such experiments alternately in the darkened room and in the concentrated sunbeam, *without moving* any portion of the apparatus in the room, and under external conditions as nearly identical as could be desired. Many preliminary experiments were made for the purpose of testing the delicacy of the arrangements, which very soon convinced me that no reliable

* From the close approximation to *identity* in the rate of consumption, it is probable that these are the same as Dr. Ure's "genuine wax-candles." He found the consumption to be, "upon an average of many experiments, 125 grains per hour." (Dict. of Arts, Manuf. et Mines, 4th Ed. Article, "Illumination, Cost of.") My experiments give respectively 136.7, 125.4, and 124.7 grains per hour. Other kinds of candles burn at a much more rapid rate.

results could be obtained unless the air was calm; and *also*, unless the candle was allowed to burn a *sufficient length of time* to establish *regularity* in the process of combustion. The days selected for the experiments were perfectly *cloudless*. The state of the barometer and thermometer was carefully noted. I now regret that I neglected to record the hygrometric condition of the air. The cone of sunlight was so directed that its *lower* margin illuminated the charred portion of the wick of the candle, while the *upper* boundary of the pencil traversed the flame near its apex. The following table presents the result furnished by *three sets* of experiments performed on as many separate days:—*

DATE.	BAR. RED'CD TO 32° F.	TEMP. OF AIR FAHR.	TIME OF CONSUMING 60 GRAINS.		AMOUNT CONSUMED IN 10 MINUTES.		DIFF. GRS.
			Dark.	Sunlight.	Dark.	Sunlight.	
May 9.	29.92	67°	26m 24s	26m 15s	22.73 grs.	22.86 grs.	—0.13
June 6.	29.72	75° 5′	28m 39s	28m 45s	20.94 "	20.87 "	+0.07
" 10.	29.62	84°	28m 55s	28m 51s	20.75 "	20.80 "	—0.05

It will be observed, that these experiments indicate *no sensible difference* in the rate of combustion of the candle in the darkened room, and in the same apartment, with a pencil of concentrated sunlight directed on the flame; *provided*, the comparison is restricted to the results

* The difficulty of keeping the pencil of solar light properly directed on a flame of *variable* altitude, induced me to try a "Burning-Fluid" lamp, having a short cylindrical reservoir, furnished with two wicks. The following results were obtained:—

Date.	Bar.	Temp.	Amount Consumed in 10 Minutes.	
June 3d	29.72	74° F.	Dark = 40.82 grains.	Sun = 40.00 grains.
" 4th	29.78	75° "	" = 40.95 "	" = 39.48 "
" "	" "	" "	" = 38.34 "	
" "	" "	" "	" = 37.52 "	

The progressive *decrease* in the rate of combustion, as indicated by the experiments of the 4th of June, shows that the variations in the *height of the liquid* are sufficient to vitiate the results obtained from burning any *fluid material*.

obtained on *any given day*. In two instances there was a slight *excess* in the rate of burning in the *sunshine;* and in the other, the excess was in favor of the dark; but the differences are probably within the limits of the experimental inaccuracies incident to such delicate investigations.

These *negative results* are the more striking from the fact that, if solar light exercised the decided influence on the process of combustion which Dr. McKeever's experiments seem to indicate, we should expect the effects to be much more marked and conspicuous, when the light was increased in intensity from *eight to tenfold* by the concentration of a lens. The fact that the rays of the sun traversed the *glass lens* before they fell on the flame can scarcely be urged as a possible explanation of the discrepancy; for Dr. M'Keever obtained analogous results when he employed lanterns (vide Exp't No. 5).

The obvious variation in the rapidity of combustion on *different days* (as exhibited in my experiments) illustrates in a most striking manner the decided influence exercised on the process, by comparatively slight alterations in the external conditions. This fact should inspire us with wholesome caution, and check the spirit of rash generalization. Throwing out of consideration the possible fluctuations in the rate of burning, arising from the want of homogeneity in the combustible materials and imperfections in the mechanical arrangements by which they are consumed, there are *three external conditions* which may be supposed to exercise more or less influence on the rapidity of the process. These are, *first*, Barometric Pressure; *second*, Temperature of the Air; and *third*, Amount of Aqueous Vapor present. I propose to consider each of these separately.

1. *Barometric Pressure.*

From *à priori* considerations, we should be led to expect that an increase of barometric pressure, through the consequent condensation of the air, would, *cæteris paribus*, tend to *augment* the rapidity of combustion by furnishing the burning matter with a greater amount of oxygen in a given volume. Unfortunately, direct experiments are wanting to test this in as satisfactory a manner as we should desire. The older experiments in the Boylean vacuum, inasmuch as they relate to the degree of rarefaction at which combustion *ceased*, do not give us information in regard to the *rapidity* of the process at the various stages of exhaustion. The same remark applies to the later experiments of Grotthuss, as well as to the admirable "Researches" of Sir Humphrey Davy on the "Effects of Rarefaction, by partly removing the Pressure of the Atmosphere upon Flame and Explosion."* The experiments of the latter show that rarefaction produces striking alterations in the size and character of the flame, but do not touch the question of the relative *rate* of burning under different pressures; they test the comparative *combustibility* of different bodies, rather than the rapidity of consumption of a *given body* under various degrees of rarefaction. Nevertheless, Davy informs us that he determined from actual experiment that the amount of *heat* developed in a *given time* by combustion is slowly *diminished* by rarefaction, "the diminution of the cooling power of the nitrogen being apparently in a higher ratio than the diminution of the heating powers of the burning bodies." Speaking of the phenomena of combustion in *condensed* air, he says: "I ascertained, however, that both the light and *heat* of the flames of the taper, of sulphur and hydrogen, were increased by acting on them by air condensed four times; but not more than they would have been by an addition of one fifth of oxygen." Again, he says: "But by compression, there can be no doubt, the heat of flames from *pure* supporters and combustible matter may be greatly increased, probably in the ratio of their compression." In the case of air, he does not think the effect would be so great. Inasmuch as the quantity of heat developed in a *given time* by the burning of a *given substance* is known to be a measure of the amount of matter undergoing oxidation, we are justified in the inference, that the foregoing results of Sir H. Davy's experiments show that the *rate* of combustion was retarded by the rarefaction, and accelerated by the condensation of the air.

The most satisfactory results in relation to the influence of *condensed air* on the process of combustion are those incidentally furnished about sixteen years ago, by M. Triger, a French civil engineer, during the operations necessary for working a bed of coal lying under the alluvium bordering the river Loire, near Languin, in the department of *Maine-et-Loire*. In traversing an overlying stratum of quicksand, from fifty-nine to sixty-five and a half feet thick, he found it requisite to

* Vide Davy's "Researches on Flame," Phil. Trans. for 1817, p. 45, et seq. Also, Works of Sir H. Davy, edited by Dr. John Davy, Vol. VI. p. 51, et seq., London, 1840

devise some means of excluding the semi-fluid quicksand and water, which found their way under every arrangement analogous to ordinary *coffer-dams*, in such quantity as to defy all pumping operations intended to keep them dry. For this purpose, M. Triger employed large sheet-iron cylinders, about 3.39 feet in interior diameter, securely closed at the top, in which — by means of a condensing pump, incessantly worked by a steam-engine — air was condensed to an amount sufficient to counteract the external hydrostatic pressure. The ingenious contrivance fully justified the expectations of the engineer; but the workmen were thus compelled to labor in air condensed under a pressure of about *three* atmospheres. Among other curious results of this state of things noticed by M. Triger were the remarkable effects of condensed air on combustion. Much annoyance was at first experienced from the *rapid combustion* of the candles, which was only obviated by substituting flax for cotton threads in the wicks.* Similar phenomena were observed, a few years ago, by the engineers of the Wilmington and Manchester Railway, who employed analogous apparatus for securing the foundations for the piers of the railroad bridge across the Great Pee Dee river, in South Carolina. So far as I have been able to ascertain, the results manifested in this case were identical with those recorded by M. Triger, and afford a most striking confirmation of the influence of condensed air in *accelerating* the process of combustion.

On the other hand, facts are not wanting to prove, that combustion is *retarded* at considerable elevations above the ocean, where the air is rarefied by diminished pressure. In a letter recently communicated to the Royal Society of London, from J. Mitchell, Esq., Quartermaster of Artillery at Bangalore, India, "On the Influence of Local Altitude on the Burning of Fuses of Shells," this officer shows, that there was a *progressive retardation* of the rate of combustion of the fuses, at altitudes of 3,000, 6,500, and 7,300 feet, as contrasted with the rapidity

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* Vide Comptes Rendus, Tome XIII., p. 884, et seq., Paris, 1841. Also, Annales de Chimie et de Physique, 3d series, Tome III., p. 234, et seq., Paris, 1841. The following are the words of M. Triger; "A la pression de trois atmosphères, cette accélération devient telle que nous avons été obligés de renoncer aux chandelles à mèches de coton pour les remplacer par des chandelles à mèches de fil. Les premières brûlaient avec une telle rapedité, qu'elles duraient à peine un quart d'heure, et elles repandaient en outre une fumée intolérable."

of burning at the artillery depôt yard. This difference Mr. Mitchell, very rationally, attributes "to the rarity of the atmospheric air, and of its constituent oxygen, at the higher stations." * The following table, in which I have reduced the barometric heights to the *freezing point*, exhibits the *mean results* of his experiments.

Height in Feet.	Bar. at 32° Fahr.	Temp. Fahr.	Average of Time of Burning 8 inches of Fuse.	No. of Exp'ts.
Depôt	29.610	89°	14.25 Seconds	6
3,000 ft.	26.755	82°	15.78 "	5
6,500 "	23.951	61°.8	17.10 "	3
7,300 "	22.979	54°.2	18.125 "	2

These experiments seem to have been made with great care; all in the presence of artillery officers, who were furnished with the most accurate methods of measuring time. They amply prove the fact, that combustion is retarded at considerable elevations.

Thus a variety of well-established facts concur in fortifying the conclusions to which we are led by *à priori* reasoning; *namely*, that the process of combustion is *retarded* by diminution of the density of the air, while it is *accelerated* by its condensation. It has long been a matter of common observation, that ordinary wood fires burn more freely when the barometer is high; but Mr. Marcus Bull and others maintain,† that this result is not owing to the augmented *density* of the air, but to the greater *dryness* of the atmosphere. The facts brought forward in this paper are strongly opposed to this explanation; for, there are not the slightest grounds for supposing, that there was *less* than the ordinary amount of aqueous vapor present in the condensing cylinders of M. Triger; or *more* than the usual quantity mixed with the air at the elevated stations in India. On the contrary, physical considerations lead us to precisely *opposite* conclusions.

* Philosophical Magazine, 4th Series, vol. 10, p. 48. July, 1855. Fuses burn *without air*; but the *rate* of burning is influenced by atmospheric oxygen.

† *Vide* Trans. of Am. Philosophical Society, 2d Series, vol. 3, p. 55, 56. Philadelphia, 1830.

2. *Temperature of the Air.*

In relation to the influence of the *temperature* of the air on the *rate* of combustion, our information is still more meagre. The experiments of Grotthuss and Sir H. Davy on the "Effects of Rarefaction by Heat on Combustion and Explosion," give contradictory results;* but, as they relate exclusively to the influence of temperature on the *ignition* of explosive mixtures of gases, they test its effects on *combustibility*, and are obviously inapplicable to the question under consideration. The well-known effects of the "hot blast," in increasing the temperature of furnaces, cannot be applied as a test of the influence of warm air on the rate of combustion, under ordinary circumstances. *First*, because the air of the "hot blast" is *not* in its natural state of *density;* and, *secondly*, because the augmentation of temperature observed in such cases, probably arises from its greater *availability*, growing out of the fact, that less heat is carried off in the products of combustion; rather than an absolute increase in the *rapidity* of burning.

In the absence of direct experimental evidence, it may be admissible to apply general reasoning based upon well-known physical principles. So far as an increase of temperature influences the *density* of the air, it is sufficiently evident, that its effects must be equivalent to a diminution of barometric pressure; and, consequently, must tend to *retard* the process of combustion. Assuming the temperature of the flame to be *constant*, it is likewise plain, that the *draught* created by it,—depending, as it is known, on the difference of temperature between the flame and that of the surrounding air,—must be *diminished* in a warm atmosphere; and, therefore, also tend to *retard* the rate of combustion, *cæteris paribus*, during hot seasons. But inasmuch as the variations in the velocity of the draught are proportional to the *square roots* of these differences of temperature, it is obvious that its effects must be insignificant under ordinary fluctuations of atmospheric temperature. For example, supposing the temperature of the flame to be 1,500° Fahr., then the fluctuation of the draught between the temper-

* Phil. Trans, for 1817, p. 53.

ature of 80° and 60° Fahr., would be in the ratio of

$$\sqrt{1500-80} : \sqrt{1500-60} = \sqrt{1420} : \sqrt{1440}, = 1 : 1.0070.$$

When, however, the comparison is made between bodies burning in summer and in winter, the influence from this cause will be more sensible, and ought not to be entirely overlooked.*

On the contrary, it is *possible* that an augmentation of temperature might tend to *accelerate* the process of combustion, by favoring the liquefaction of the wax, and, perhaps, facilitating the oxidation of the combustible matter. If any such influence is exercised, it is probable, however, that its effect must be inappreciable under ordinary circumstances. Under this view of the subject, the only obvious influence which atmospheric heat exercises on the rapidity of combustion is connected with its effects on the *density* of the air; and that, consequently, an increase of temperature should, *cæteris paribus*, *retard* combustion, and *vice versâ*.

3. *Amount of Aqueous Vapor present.*

Sir Humphrey Davy found that "a very large quantity" of *steam* was required to prevent sulphur from burning; that an explosive mixture of oxygen and hydrogen, when mixed with *five times its volume of steam*, still exploded by the electric spark; and that a mixture of air and carburetted hydrogen gas required "a *third* of steam to prevent its explosion, whereas one fifth of azote produced the effect." † Under any point of view, it is obvious that the presence of aqueous vapor can only tend to *retard* the process of combustion: first, because it diminishes the amount of oxygen in a given volume of air, and secondly, because an admixture of any inactive gas tends to extinguish the burning body, as is abundantly proved by the experiments of Sir H. Davy and others. When vapor is present in large quantities, there can be no doubt of its controlling agency on combustion. This is illustrated by the successful application of the plan proposed by M. Dujardin, of Lille, in 1837, for extinguishing fires occurring in steamships, by per-

* I endeavored to test the influence of *temperature* on the rate of combustion, by placing the burning candle over a *large heated plate;* but, as might have been expected, the *unsteadiness of the flame* rendered the experiment unsatisfactory.

† Phil. Trans. for 1817, p. 65.

mitting the steam from the boilers to escape into the apartment in which the combustion originates.* But experiments are still wanting for determining its influence on the rate of burning, when existing in the small quantities in which it is usually associated with the atmosphere.† The experimental researches of Mr. David Waldie, in relation to the mixture of various gases with air, led him to the *general law*, that, "of incombustible gases which remain undecomposed, the power of preventing combustion is in the order of their density;" and that "this effect of density in cooling the flame depends on the excessive *diffusion* of the flame in the denser gas."‡ Under ordinary circumstances, the *density* of the aqueous vapor existing in the air is comparatively *small*, so that, according to Mr. Waldie's law, its influence on combustion ought not to be very striking. It is very desirable that this point should be submitted to a more rigorous experimental investigation.

Having discussed the probable influence of the *three external conditions* on the rate of combustion, we are, in a measure, prepared to investigate their *adequacy* to explain the variations in the *rapidity of burning*, as indicated by the experiments which I have brought forward. In *none* of them have we the observations necessary for ascertaining the *hygrometric condition* of the atmosphere; this must, therefore, be thrown out of consideration. In Dr. M'Keever's experiments the barometric indications are *not* given; neither is it known how many of them were performed on any *one day*. In my experiments, as well as in those of Mr. Mitchell, we are furnished with the data requisite for estimating the *combined* influence of *pressure and temperature*. Assuming, with Sir H. Davy, that the *rapidity* of combustion is in the *direct ratio of the density of the air*, we may submit these two effects to a *quantitative* estimation, by using Mariotte's law and Regnault's co-efficient of expansion for air.

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* Comptes Rendus, Tome V., p. 28, Paris, 1837; also, Tome XXXV., p. 368, et 706. Paris, 1852.

† The curious results obtained by Mr. J. F. Dana, and subsequently by Mr. Samuel Morey, in relation to increasing the *brightness* of the flames of highly carbonaceous combustibles, by throwing a jet of steam into them, are obviously inapplicable to candles (vide Silliman's Journal, 1st series, Vol. I., p. 401; Vol. II., p. 116, 122; and Vol. VII., p. 141).

‡ Vide Philosophical Magazine, 3d series, Vol. XIII., p. 86 et seq., August, 1838.

1. Presuming that *each set* of experiments made by Dr. M'Keever, alternately in the dark and in the sunshine, was performed on the *same day*, and therefore under *identical* barometric conditions, we may form some estimate of the adequacy of *temperature* to account for the difference in the rate of burning observed by him. The subjoined table, which I have constructed from the data previously given, will place this in a clear light.

Experim't.	Temp. Dark.	Temp. Sun.	Consumed in ten minutes		Ratio of Amount in Sun & Dark.	Ratio of Den. Air in Sun & Dark.
			in Dark.	in Sun.		
No. 1	67° F.	78° F.	18.50 grs.	17.00 grs.	1:1.088	1:1.021
" 2	67° "	78° "	15.72 "	14.29 "	1:1.100	1:1.021
" 3	68° "	80° "			1:1.053	1:1.023
" 4	67° "	79° "			1:1.111	1:1.023

The remarkable discrepancies indicated by the numbers in the first column of *ratios* afford a striking illustration of the existence of some disturbing cause, tending to vitiate the accuracy of these experiments. But a glance at the numbers contained in the two columns of *ratios* is sufficient to show, that *temperature alone* is entirely *inadequate* to account for the diminished rate of combustion in the sunshine. A remarkable difference is observed in the *rate* of consumption in experiments 1 and 2. No. 1 was made with a "green wax taper," and No. 2 with a "taper;" but, as from the context, the *second* experiment appears to be a repetition of the *first*, the presumption is, that the same kind of taper was used in both cases. The rate of burning in experiments Nos. 3 and 4 was determined by the *time* required to consume a *given length;* and as one of them was made with a mould candle, and the other with a taper, no comparison can be extended to them, so far as the rates of consumption in these two cases are concerned. The irregularities exhibited in these results most probably arose from the agitations of the atmosphere, which were incident to the method of exposing the burning body to the sunshine in the *open air*. As the *excess* of consumption in the dark varied from 5 to 11 per cent., whereas the excess in the density of the air was only 2.3 per cent., it is evident that some *other* cause than *temperature* must be evoked to explain the difference.

2. In my experiments, the conditions were such as to eliminate the effects of *temperature* on the results obtained in the dark and in the sunshine on any given day; and it has been shown, that for *each pair* of experiments thus conducted, the variations in rate of combustion *do not* exceed the probable limits of experimental error. In this case, therefore, the question to be determined is, whether the differences in the *rapidity of burning* observed on *different days* can be explained by the variations of the barometer and thermometer? For this purpose, I shall take the average of each *pair* of experiments, as a nearer approximation to the correct rate of burning on each of the three days. The following table, in which the *relative densities* of the air have been calculated by *combining* the effects of barometric and thermometric oscillations, will serve to illustrate this point: — *

Ratio of Consumption.	Ratio of Density of Air.	Difference.
In Exp'ts 1 et 2 = 1 : 1.0904	1 : 1.0230	+ 6.74 per cent.
" " 2 et 3 = 1 : 1.0063	1 : 1.0193	— 1.30 " "
" " 1 et 3 = 1 : 1.0967	1 : 1.0428	+ 5.39 " "

It will be seen, that the rate of combustion increases in a decidedly *higher ratio* than the *density* of the air. If, therefore, we assume that the rapidity of burning is, *cæteris paribus*, in the direct ratio of the density of the air, it follows that some *other agency* must have coöperated in these cases.

3. The results of Mr. Mitchell's experiments, at different altitudes, may, in like manner, be subjected to a similar numerical test. The subjoined table is constructed from the data contained in that which is given on a previous page.

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* In making these calculations I used the following formula, based upon the two well-known physical laws, that the *density* of any permanent gas varies *directly* as the compressing force, and *inversely* as the volume: —

$$d : d' :: \frac{b}{1 + 0.002036\,(t - 32)} : \frac{b'}{1 + 0.002036\,(t' - 32)};$$

in which d and d' represent the *densities*; b and b' the *barometric heights* reduced to the freezing point; and t and t' the *temperatures* on Fahrenheit's scale.

Ratio of Rate of Burning.	Ratio of Density of Air.	Difference.
In Exp'ts 1 et 2 = 1 : 1.1074	1 : 1.0926	+ 1.48 per cent.
" " 2 et 3 = 1 : 1.0836	1 : 1.0755	+ 0.81 " "
" " 3 et 4 = 1 : 1.0599	1 : 1.0270	+ 3.29 " "
" " 1 et 3 = 1 : 1.2000	1 : 1.1751	+ 2.49 " "
" " 2 et 4 = 1 : 1.1486	1 : 1.1045	+ 4.41 " "
" " 1 et 4 = 1 : 1.2719	1 : 1.2068	+ 6.51 " "

This comparison places in a still stronger light the fact, that the augmentation in the rate of burning increases in a somewhat *higher ratio than the density of the air;* while, at the same time, it clearly demonstrates the *controlling influence of atmospheric density* on the phenomenon of combustion. The *extreme rates* of burning are as the numbers 100 to 127, while the corresponding *densities* of the air are as 100 to 121, nearly; in the other cases, the approximation to *identity* in the ratios is still closer. Would the variations in the hygrometric state of the atmosphere, — which we have left out of consideration, — explain this discrepancy? In the absence of the experiments necessary for testing this question, it would be premature to hazard any conjecture. I may remark, however, that in the case of Mr. Mitchell's experiments, the correction for the effects of aqueous vapor would, probably, *in one point of view,* operate in the *wrong direction,* and thus tend to increase the discrepancy in the ratios. For, as the temperature was decidedly higher at the lower stations, it is more than probable that the *tension of vapor* was *greater* there than at the upper ones; and, consequently, that its influence in retarding combustion should be *relatively greater* at the points nearer the sea-level. This, of course, would tend to *equalize* the rates of burning at lower and higher altitudes, when no correction is made for this cause. On the contrary, it is obvious that the influence of vapor having a *given tension,* in altering the relative amount of air in a *given volume,* must be *greater* when the barometer is low. From this cause, the aqueous vapor at the upper stations might have had a *greater* effect in retarding combustion, and thus tended to *exaggerate* the difference in the rates of burning.

The comparatively large rate of consumption indicated by my *first* experiment of the 9th of May, (being more than 9 per cent. above the others,) was most probably attributable to a combination of causes. All of the *three external conditions* concurred in accelerating the process. The barometer was *high*, the temperature *low*, and the atmosphere *excessively dry*. The last-mentioned condition was accidentally forced upon my attention, from the fact, that on that day I *failed* in an experiment for determining the dew-point by means of Daniell's hygrometer.*

From the foregoing discussion it is evident, that the subject demands a thorough experimental investigation, with a minute attention to all of the external conditions which may influence the results. This I propose to undertake during the next twelve months. In the mean time, it is hoped, that these preliminary researches may prepare the way for a clearer appreciation of the difficulties which are to be encountered.

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* Collaterally related to this subject are the effects of condensed and rarefied air and of temperature, on the *process of respiration and the elimination of carbonic acid*, in men and other warm-blooded animals. M. Legallois found, that when warm-blooded animals breathed air under *pressure*, reduced to 11.811 inches, the amount of oxygen consumed was *diminished*. (Ann. de Chimie et de Phys., Tome 4, p. 113. 1817.) M. Theodore Junod's experiments show, that *condensed air* produced deep inspirations and an agreeable glow throughout the system; while *rarefied air* had an opposite effect. (Archives Générales de Médecine, 2d Series, Tome 9, p. 157. Paris, 1835. Also Magendie's Report on the same Memoir, Comptes Rendus, Tome 1, p. 60. Paris, 1835.) The observations of M. Triger, already referred to, indicate analogous effects on those who labored in the condensed air. They could do double work without fatigue; and even old asthmatics seemed to recover their vigor. (Comptes Rendus, Tome 13, p. 884 et seq. Paris, 1841.) M. Vierordt tested the effects of barometric pressure between 29.309 and 30.197 inches. The *average* rise of 0.5036 of an inch,

Increased the air expired, 35.746 cub. inches per minute.
" " No. of respirations 0.74 " "
" " " pulse 1.30 " "

Dr. Hutchinson found, that in a mine 1,488 feet deep, where the pressure was 1.64 inches more than at the sea-level, the respiration was *increased* 2.4 per minute, and the pulse 1.3 per minute. (Cyc. of Anat. et Physiol. Art. Respiration, vol. 4, p. 348, 349. London, 1852.)

Analogous effects are produced by *temperature*. In the famous experiments of Séguin and Lavoisier, at 82° Fahr., the former (fasting and at rest) consumed 1,210 French cubic inches of oxygen per hour; whereas, at 57°, he consumed 1,344 cubic inches per hour. (Mémoires de l'Acad. Royale, for 1789.) Dr. Crawford found, that a Guinea pig, at 55°.5 F., abstracted *twice* as much oxygen from the air as at 104° F. (Expts. et Obs. on Animal Heat, 2d ed. p. 311–315. London, 1788.) Dr. W. F. Edwards found that birds consume more oxygen in winter

Perhaps, however, in the present stage, we may be warranted in dedu cing *two* conclusions: FIRST, That *solar light does not* seem to exercise any sensible influence on the process of combustion; and, SECONDLY, that variations in the *density of the air do exert* a striking effect in retarding or accelerating the rapidity of the process; the rate of burning augmenting with every increment of density, and *vice versa;* but the exact ratio between them remains to be determined.

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than in summer. (De l'Influence des Agens Physiques, sur la Vie, chap. 6, p. 195. Paris, 1824.) The best experiments are those of M. Vierordt. (*Op. cit. supra.*) He obtained the following results between 37°.4 and 75°.2 F.: —

	Av. Temp. 47°.24	Av. Temp. 66.92	Diff.
Pulse per minute	72.93	71.29	1.64
Respiration " "	12.16	11.57	0.59
Vol. air expired " " in cubic inches	407.00	367.00	40.00
" CO_2 " " " " "	18.25	15.72	2.53

M. Felix Letellier's experiments on warm-blooded animals confirm these results. He found the amount of *carbonic acid* evolved *per hour*, at different temperatures, to be as follows: —

	86° to 104° F.	59° to 68° F.	32° F.
Canary	0.129 grammes	0.250 grammes	0.325 grammes
Pigeon	0.366 "	0.684 "	0.974 "
Two Mice	0.268 "	0.498 "	0.531 "
Guinea Pig	1.453 "	2.080 "	3.006 "

(*Vide* Comptes Rendus, Tome 20, p. 795. Paris, 1845. Also Ann. de Chimie et de Phys. 3d Series, Tome 13, p. 478. Paris, 1845.) Doubtless *physiological reactions* exercise a powerful influence over the results of such experiments; nevertheless, as respiration is *essentially* a process of combustion, they have a *general bearing* on the question under consideration.

2. On the Vibrations of the Fall over the Dam at Holyoke, Mass. By Prof. E. S. Snell, of Amherst, Mass.

The vibrations of dams and waterfalls must have attracted attention long ago; but I find no mention of such facts in scientific records, except in an article by Professor Loomis, (Sill. Jour. Vol. XLV. 1843.) In that paper, seven instances of vibrating dams are cited, in all of which the oscillation is attributed to the friction of the water on the edge of the dam, operating like a violin bow across the edge of a tumbler. The considerations which Professor Loomis adduces in proof are, I think, sufficient to establish the correctness of his conclusion in the cases he has described. But I am satisfied that there is another class of vibrating waterfalls, in which the pulsations are owing to a wholly different cause.

The only example of a vibrating fall which I have had opportunity to observe personally is that of the Connecticut over the dam at Holyoke. The whole river, except the comparatively small part taken off in canals, falls in an unbroken sheet 1,017 feet long, between piers, and 30 feet high, and varying in depth on the top of the dam from *six inches* in low-water, to *ten or twelve feet* in high-water. The dam is necessarily built in the strongest manner, and firmly secured to the rocky bed, which extends entirely across the channel. In looking down from the pier at either end, the edge of the entire dam is plainly seen through the smooth bending mass of water, and appears perfectly straight, and without a sign of tremor. It would be difficult to convince the spectator that the strong pulsation which he sees and hears in the fall, and, in some places, feels in the air, can be caused by invisible vibrations in the dam. If the visitor descends to the opening in the pier, and stands at the end of the vacancy behind the sheet, he perceives the air rushing in and out alternately with a strong puffing motion.

I consider the column of air between the fall and the dam to be the original vibrating body, set in motion by the descending sheet. The water, in all cascades, carries with it into the basin below a portion of the adjacent air, both in front and behind, which appears afterwards in foam on the boiling surface. This action of the water produces a rarefaction behind the sheet, which is a sufficient exciting cause of the vibrations. In high cascades, like those of Niagara, there is produced by the same cause a *current* of air, setting *inward* at the top, and *outward* at the bottom; so that a continual circulation is maintained at each extremity of the fall. But in a tube of air more than a *thousand* feet long, and scarcely *twenty* feet in diameter, there is not room for free *circulation*, and a *pulsation* takes place instead.

I visited the fall on the 25th of July last, and again on the 29th, and repeatedly counted the vibrations, both by the eye and the ear. The temperature at the time of my first visit was about 80° F.; and I made the number of vibrations 136, 137, or 138 per minute, and the mean rate 2.28 vibrations per second. During the second visit, the temperature fell from near 80° to about 70°, as I judged; and the number of vibrations diminished from 137 to 134; i. e. from the rate of 2.28 to 2.23 per second. As there is an opening of eight feet by three feet through the pier at each end, and as the sheet is diverted from the pier some four or five feet by means of an apron at the top of the dam, I regard the space behind the fall as an organ pipe open at both ends, 1,008 feet long, or nine feet less than the distance between the piers (= 1017 ft.) Applying to it the formula $N = n. \frac{V}{E}$, we have $2.28 = n \times \frac{1145}{1008}$, for the temperature of 80°; or $2.28 = n \frac{1133}{1008}$ for 70°; or $n = 2$ very nearly. Hence it appears that the air, in an open tube of this length, if two nodes were formed within it, would vibrate at the observed rate, — about $2\frac{1}{4}$ times per second.

My attention was of course directed to the inquiry, whether there was any indication of nodes in the flexure of the sheet. But here I found an irregularity and a changeableness which were perplexing. At my first observation, there seemed to be three segments in motion; that next the left bank extending nearly half across the river, and the other two dividing the remainder of the breadth about equally. The vibrations of each two successive segments were alternate with each other. But this arrangement was sometimes confused, and it became difficult to trace any divisions. At the time of my next visit, the segments were, at first, three in number, as before, but now of equal length; and the points of division quite plainly marked by their small motion, and by the small height to which the spray was tossed at each outward swing of the fall. But before I left, (about two hours after,) there was as distinctly seen a division into *two* equal segments, with the greatest motion in the centre of each. This last was the mode of vibration which I had expected to see, as indicative of two nodes; for I supposed the water would suffer the greatest transverse disturbance where the inclosed air changed its density most; i. e. at the nodes; and that it would vibrate least where the density of the air was constant; in other words, at the middle of the ventral segment.

In times of high-water, the oscillations of the fall become invisible; probably because the mass is too great to be perceptibly swayed by the difference of internal and external pressures. At the times of my observations, the depth of water above the dam was near *two* feet; and the foot of the sheet seemed to vibrate through *eight* or *ten* inches.

I am strengthened in my belief of the correctness of the foregoing explanation, by a conversation which I held on the subject with Professor Schaeffer, a gentleman now connected with the Patent Office in Washington. He remarked, that he had observed, in many instances, the production of a musical tone by the small cascades in the rapids of rivers; and that the tone was lower as the cascade was broader; so that he was accustomed to consider the cavities behind such falls as organ pipes, yielding various musical sounds, according to their length.

The vibrations of Holyoke Fall are generally noticeable in Holyoke and the village of South Hadley Falls, and are occasionally perceived at the distance of several miles. At the distance of half a mile, I counted the vibrations of a window sash, and found the number 137, the same as I had, an hour previous, counted in the fall itself. I have noticed the movement of doors occasioned by the fall, in a dwelling three miles distant; and am told that the effect is sometimes observed in Springfield, at the distance of eight or ten miles.

Some days after the foregoing was written, I had the opportunity of observing the vibrations for a few minutes, and found that a striking change had occurred. The water was lower than before, and the number of vibrations was nearly doubled, there being from 256 to 260 per minute, or 4.3 per second. This rate corresponds tolerably well with the supposition of *four* nodes. But, in the short time of my stay, I could not determine by the eye the fact of four vibrating segments in the sheet, though I could plainly perceive that certain parts of it had much motion, while others were nearly at rest.

If I may rely upon my recollection of what I noticed some years ago, I saw the same fall vibrating 69 or 70 times per minute; though, as I made no record at the time, it is possible that I counted the number produced in a *half* minute.

It seems quite probable that there are in the Holyoke Fall, at least three modes of vibration, corresponding to three successive octave notes, though the vibrations are too slow to produce on the ear the effect of a musical sound. The depth of water is doubtless the principal circumstance, but perhaps not the only one, which determines the mode of division in the air column. At the temperature of 80°, a column of air in an open tube, with *one* node, should vibrate 68 times in a minute; I think I once counted 69 or 70. With *two* nodes, the calculated number is 136; I made by observation, 137, as a mean result. With *four* nodes, at 70°, calculation gives 269 vibrations, observation, 258.

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3. On the Electrical Hypotheses of the Aurora Borealis. By Prof. Denison Olmsted, LL. D., of New Haven.

In a paper which the Smithsonian Institution did me the honor to publish in a late volume of their "Contributions," *On the Recent Secular Period of the Aurora Borealis*, I attempted to present a synopsis of the facts, as exhibited in the remarkable series of auroras, which commenced in 1827, and lasted for more than twenty years. I also, from a review of those facts, and from an extensive comparison of them with those witnessed in other great exhibitions of the same phenomenon, in other countries and in past ages, endeavored to state the *laws* of the Aurora Borealis in a more systematic and definite form than had been done by any other observer, within my knowledge. Finally, with these data before me, I proceeded to discuss the question of the *Origin and Cause of the Aurora Borealis*. Contrary to the opinion which ascribes it to terrestial agents, as electricity or magnetism, I argued that the origin of the Aurora Borealis is *cosmical*, the matter of which it is composed being derived from the planetary spaces.

I inferred the cosmical origin of the material of which the Aurora is composed from the following arguments: First, from the *extent* of the exhibitions, sometimes spreading from east to west for many thousand miles, and reaching to a height of a hundred miles or more above the earth, quite above the region of atmospheric precipitations; secondly, from the fact that in places differing many degrees in longitude, the different stages of the Aurora, (such as the beginning, maximum, and end,) *occur at the same hour of the night*, indicating that a place on the earth, in its diurnal revolution, comes successively under the nearest point of the auroral body situated in space; thirdly, from the *velocity* of the motions, being too small for light itself, and too great to result from any terrestrial force, as magnetic or electric attractions, occasioning a translation of the matter of the Aurora; and, fourthly, from the *periodicity* of the Aurora, especially its *secular* periodicity, appearing, as it does, at long but nearly definite intervals in a grand series of exhibitions, which increase to a maximum, and then diminish in number and intensity, until the phenomenon, in its grander forms, vanishes from our nocturnal sky,— a fact which appears to me to remove it from the pale of terrestrial, and to bring it clearly within the domain of astronomical, causes, implying a nebulous body in the planetary spaces from which the material of the Aurora is derived, having a revolution around the sun, and a period in a nearly simple ratio to the earth's period.

At a meeting of the American Association at New Haven, in 1850, I submitted an outline of the paper since published in the Smithsonian Contributions, and ventured then to declare my belief, founded on the history of the phenomenon, that the brilliant exhibitions of the Aurora Borealis, which for more than twenty years had very often presented astonishing displays in our nocturnal heavens, were nearly over, and would soon cease, and not appear again in the same intensity and frequency until after an interval of forty years or more; the interval from the *maximum* of one secular period to that of another being estimated, from historical records, to be from 60 to 65 years. I would now respectfully ask the members of the Association to remark, that for five or six years past, the number and splendor of these exhibitions have in fact greatly diminished, compared with the period marked off in my paper as the "Secular Period;" and I would solicit their further attention to the subject, in order to see how far the expectation I ventured to intimate in 1850 shall be fulfilled.*

In my paper published by the Smithsonian Institution, while full space was liberally allowed me to express my own opinions as to the origin and cause of the Aurora Borealis, it was deemed inconsistent with the plan of the Smithsonian "Contributions," to admit of the discussions of the opinions of others. Some remarks, therefore, which I had made on hypotheses urged by different writers, in explanation of the Aurora, were not published with the article referred to. These, so far as relates to the electrical hypotheses, I now beg leave to submit to the present meeting.

After the discovery of the identity between electricity and lightning, and the consequent connection of electricity with thunderstorms, it became the practice of the interpreters of nature to ascribe every thing mysterious and not otherwise accounted for, to this wonderful agent.† No student of nature can indeed doubt, that electricity holds a most important place among the ultimate causes of physical phenomena; we only complain of the practice of referring effects to this cause simply on the ground of its known activity, without either proving its presence, or legitimately deriving those effects from its known properties. One evil consequence of such a practice has been, to give a qui

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* Those who are not old enough distinctly to remember the great auroral exhibitions of 1837 and the years immediately following, will be liable to class among "brilliant Auroras," displays of a kind greatly inferior to those in which such sights as the following were not unusual. First, before the end of twilight, the northern sky presented a bright illumination, as though the sun were rising in that quarter. Soon there appeared, stretching along the northern horizon, and rising a few degrees above it, a bank of luminous vapor, often more or less obscured by a smoky hue, the whole alive with flickering motions. In due time, but often from 10 to 11 o'clock, a forest of silvery spindles (*streamers*) would shoot up from the bank of auroral vapor, and crimson columns begin to form simultaneously in the north-east and north-west, which would rush upward, in company with innumerable streamers, to a point a little south-east of the zenith, — the pole of the dipping-needle, — around which they would arrange themselves in a magnificent *corona*. Frequently, at a later hour, *auroral waves*, of surprising appearance and incredible velocity, would roll up towards the zenith, which frequently continued until the dawn of day.

It is only such exhibitions as these that are entitled to be denominated "Grand Exhibitions of the Aurora Borealis."

† See Beccaria, Priestley, Morgan, Encyclopædia Britannica, etc.

etus to any further investigation of the subject, supposing that when it was determined to be an "electrical phenomenon," there was no need of any further inquiry into the cause. Such has been particularly the case in respect to explanations of the Aurora Borealis. To begin with Dr. Priestley: "That the Aurora Borealis is an electrical phenomenon (says the Doctor in his elaborate History of Electricity), was, I believe, never disputed from the time that lightning was proved to be one." * The author urges scarcely an argument in favor of this hypothesis, but deems it a sufficient explanation of the Aurora to call it an electrical phenomenon. Other writers, however, have arrayed their arguments in favor of this hypothesis in form. Thus Morgan, in his able Lectures on Electricity, published in England in 1794, urges the following reasons for the opinion, that the Aurora Borealis arises from great quantities of electricity discharged through the upper regions of the atmosphere. "If," says he, "thunder be caused by the electric fluid, there is a certain height in the atmosphere in which the cause of thunder must necessarily assume the appearance of Northern Lights. In air rarefied to a certain degree, the passage of the electric fluid is attended with all the undulating coruscations of the Aurora Borealis. Indeed, there is not a single circumstance in the experiment of passing the spark through an exhausted tube, which does not bear a resemblance to something observed in the Northern Lights. There is the same peculiarity in their motion, the same variety in their color, and the same quick alternation of flashes in both; the streams are alike vivid and pointed; and if the exhaustion be properly managed, some parts will appear with that reddish tinge which is often observed in the air by the vulgar with fear and consternation." †

Upon this argument I remark, that this writer, like all other writers who have insisted on the resemblance between the appearance of the Aurora Borealis and the electric spark passing through rarefied air, has greatly overrated the degree of resemblance. That luminous appearances in one case should resemble luminous appearances in another case is nothing remarkable. A fire, a lamp, the sun, the stars, have all some points of similarity. Now, after multiplied comparisons of the appearances of the Aurora with those of the electric spark in rare-

* History of Electricity, p. 376.

† Morgan's Lect's on Elec. II., 237. Singer's El'm. of Elec., 251.

fied air, as exhibited in what is called the auroral tube, I am compelled to say that I think the likeness is very faint, both in regard to the shades of color and to the peculiar motions. There is nothing in the auroral tube experiment which resembles the auroral streamers or arches or waves or corona. Between the *flashes* of the aurora and those of the spark in the tube, there is occasionally some resemblance. But light derived from very different sources often exhibits many points of similarity, and it is unsafe to predicate upon such incidental resemblances an identity of origin. Reasonings of this nature were often exemplified by the earlier writers on Natural Philosophy. Thus Wallis, the preceptor of Newton, accounts for thunderstorms by ascribing them to the accidental meeting in the air of the elements of gunpowder; an explanation which was countenanced by the striking resemblance of lightning and thunder to the flash and explosion of gunpowder.

The same author proceeds: "As the rarefaction of the air increases, so does the striking distance of the charge that passes through it. If two clouds, therefore, the one positive and the other negative, should have no other circuit, is it not highly probable that they will discharge themselves through the higher regions of the atmosphere?" The nature of the reasoning appears to be this: Lightning is known to be owing to the discharge of electricity from one cloud to another differently electrified. Now, from experiments with the electric machine, in passing electricity through a tube containing rarefied air, we know that in proportion as the air is more rarefied, the further the electric spark will pass, and the more feeble will be its light, and consequently more like that of the Aurora Borealis. May we not then suppose that this phenomenon arises from the flowing of electricity from one distant cloud to another, through the upper regions of the atmosphere, where the air is much rarefied? But let us remark upon what feeble grounds this analogy is predicated. Thunder clouds are not usually more than a mile high; the auroral arches, sometimes at least, are seventy or one hundred miles and more. The passage of lightning from one cloud to another is seldom more than a mile or two in length; the same auroral exhibition, as that of November, 1848, has been known to extend from Western Asia over Europe and America to California, covering a large part of the northern hemisphere.

The *crackling or hissing noise* with which the Aurora Borealis is said to be sometimes attended is alleged in proof of its electric origin. The existence of any such sound proceeding from an auroral exhibition is doubtful; but its existence being granted, the supposed sound is very unlike that produced by electrical explosions; and, were the resemblance more striking than it is, the presence of electricity would not necessarily be implied, inasmuch as the sound in question might arise from various other causes, such, for example, as the rapid passage of auroral vapor through the atmosphere.

The foregoing considerations lead me to think that any reasoning which ascribes the Aurora Borealis to electricity, founded on a supposed resemblance between the sensible appearances of the Aurora and known appearances caused by electricity, is inconclusive and generally fallacious; yet this was the mode of argument generally adopted by those who first devised the hypothesis, including most writers on the subject of the age of Priestley. But later defenders of the electrical hypothesis have relied little on these arguments, nor have they agreed among themselves in any thing except in the vague idea that the Aurora Borealis is, *in some way or other*, caused by electricity, while they have differed widely among themselves in regard to the *modus operandi* of this agent. Thus nothing can be more unlike the explanation already recited as that adopted by Beccaria, Priestley, Morgan, and their followers, than the explanations subsequently put forth by Biot, Hare, and De La Rive, men whose philosophical reputation entitles their opinions to the greatest respect, although we may venture to examine them with the freedom allowable in an inquiry after truth.

In the year 1817, M. Biot witnessed at the Shetland Islands, where he chanced to be residing, a great auroral exhibition, of which he has given a minute description in his Précis Élémentaire De Physique, accompanied by various speculations concerning the laws and the origin of the phenomenon. He points out the true mode of determining whether any appearance in the sky is within the atmosphere or beyond it, by observing whether or not it partakes of the diurnal revolution; whether it accompanies the stars from east to west, or is left behind by them; and he comes to the conclusion that the auroral exhibition takes place within the atmosphere. But the test suggested by Biot does not reach the question, which respects the *origin* of the Aurora; for if it is proved that the *phenomena* are exhibited in the atmosphere, still the question returns, was the material of the Aurora, (or what we have ventured to denominate *auroral vapor,*) derived from the earth or the atmosphere itself, or did it come into the atmosphere from the regions of space?

The author proceeds to account for the formation of arches and coronas, out of parallel columns, by the principles of perspective, in the same way that Dalton, and even Roger Coates,* at a much earlier period, had done before him. The columns, he thinks, consist of finely divided metallic matter; and being magnetic, they spontaneously arrange themselves in a direction parallel to the magnetic meridian, and of course parallel to one another, as though they were real magnetic needles. Now such columns would be conductors of electricity; and since the electricity of the lower portions of the atmosphere is (according to him) usually vitreous, as is ascertained by the electrical kite, and the upper portions resinous, as is found by æronauts, the two extremities of these vertical columns will commonly be in opposite states, and a continual current of electricity will flow through them, differing in quantity with the varying intensity, and the condition of the medium; but when the opposite states of the upper and lower extremities of the columns are very intense, the electricity, in forcing its way through the imperfectly conducting medium, gives flashes, and the other luminous appearances of the Aurora.

Although some of the appearances of the Aurora Borealis are well explained on the hypothesis of Biot, yet there are others which it hardly reaches, and others with which it appears to me to be inconsistent. There is a high degree of improbability, that such an amount of metallic vapor, derived from any *terrestrial* source, should be suddenly diffused through the atmosphere, as to cover no inconsiderable portion of the whole earth, and extend to the height of one hundred miles or more. Moreover, the motions observed in the auroral lights, although exceedingly rapid, are still progressive, and not instantaneous, as would be the case with electric flashes. Nor does the hypothesis furnish any reason for the periodicity of the Aurora, which is one of its most important characteristics.

Dr. Hare takes quite a different view of the *modus operandi* of electricity in the production of the Aurora Borealis. He considers the relation between the earth and the upper regions of the atmosphere,

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* See Phil. Trans. *Abridged,* Vol. VI.

(which severally constitute two good conductors,) as analogous to the relation between the inside and the outside coating of a Leyden jar, separated from each other by the intervening glass. "Thus," (says he,) "the atmosphere is situated between two oceans of electricity, of which the tension may often be very different. Between these electric oceans, the clouds floating in the non-conducting air must act as movable insulated conductors; and from the excitement consequent upon induction, from chemical changes, or from their proximity to the celestial electric ocean, must be liable to be electrified differently from each other, and from the terrestrial electric ocean. The Aurora Borealis (he adds) may arise from discharges from one ocean to the other of electricity, which, not being concentrated by its attraction for intervening clouds, within air sufficiently dense to act s an electric, assumes the diffuse form which characterizes that phenomenon." *

If the Aurora Borealis were formed in this way, ought it not to prevail most where the supposed electrical oceans exist in their greatest intensity, namely, in the equatorial regions; whereas there they are scarcely known, while they are at a maximum in the polar regions, where, if the analogy exists at all, they are in their minimum state of activity. We need not insist on the inadequacy of the hypothesis to account for the *production* of the matter of the Aurora itself, or for the progressive motions to which it is subject, which are too rapid to be occasioned in the manner supposed by the hypothesis; namely, by electrical "convection;" and yet too slow to consist of mere flashes of light. Nor need we urge the want of application of this hypothesis to the periodicity of the phenomenon; or to the vast extent of its simultaneous appearance; or of its commencing, reaching its maximum, and ending, at the same hours of the night in places widely differing in longitude.

M. De La Rive, of Geneva, has more recently put forth a still different form of the electrical hypothesis of the Aurora Borealis.† According to this distinguished philosopher, the electricity of the atmosphere owes its origin to an unequal distribution of temperature through the different atmospheric strata. He argues that through every sort of body heated at one of its extremities and cooled at the other, the posi-

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* Amer. Jour. Science, XXXII. 157.

† Bibliothéque Universelle, *New Series*, 3, 17.

tive electricity goes from the hot to the cold part, and the negative electricity in the opposite direction. Hence the lower end of an atmospheric column, being in contact with the earth, is constantly negative. The intensity is greater as the difference of temperature is greater; and it is greater, therefore, in summer than in winter, and greater in the equatorial than in the polar regions. The ground being rendered negative by contact with the negative columns, while the upper extremities are positive, there will be a tendency of the electricity to flow down from the top to the bottom, that is, from the upper regions to the earth, which tendency will be greater as the conducting power of the medium is greater, varying with the degree of humidity. On account of the greater humidity of the air over the poles, and consequently its greater conducting power, the electricity flows from the upper portions of the atmosphere within the torrid zone, each way to the north and south poles; and the earth being a good conductor of electricity, as is shown by the electric telegraph, where the ground is made the medium of discharge between two poles at a great distance from each other, the electricity conveyed to the poles through the upper regions, returns to the torrid zone through the earth.

In applying these principles to the Aurora Borealis, the author remarks, that it is the effect of the luminous electric currents, coursing the upper regions of the atmosphere towards the north pole, a consequence of the union of several unusual circumstances. When the sun has left the northern hemisphere, the vapors condense around the pole, and electricity flows in unusual quantities from the equatorial regions. The auroral vapor itself he considers as nothing more than the *icy particles* which are known to float in the upper regions of the atmosphere, sometimes producing lunar and solar halos; their presence in those regions being also affirmed by the testimony of æronauts.*

Upon the views of M. De La Rive I offer the following remarks. First, facts do not warrant the supposition that there is any such tendency of electricity to the poles as the hypothesis assumes, but they indicate just the contrary. Thunderstorms never occur beyond the latitude of 75°, and rarely beyond 65°, while they are most of all frequent and violent in the equatorial regions, where Auroras are seldom

* De La Rive's Letter to M. Arago, An. de Chim. et de Phys., March, 1849.

seen. Moreover, since the resistance to the passage of electricity *upward* is diminished as the air grows more rare, so much so that some writers (as Dr. Hare), maintain that the upper regions of the atmosphere correspond to the negative side of the Leyden jar, would not the supposed currents find a shorter route to an equilibrium than to go from the equator to the pole? Again, the auroral movements are progressive, and not instantaneous, like those of electricity. We may ask, moreover, are icy or snowy particles, like those which are here supposed to constitute the auroral vapor, ever known to become permanently luminous by being electrified? Are they ever known to be thus rendered magnetic, like the auroral columns? Are they known to reach to the height which, sometimes at least, the auroral matter attains, as, for example, one hundred miles above the earth? Can they be supposed to be all at once developed in such quantities as to overspread so great an extent of the earth as the Aurora sometimes covers, reaching, for instance, from Western Asia to California? Will the hypothesis account at all for the periodicity of the Aurora? Believing that a negative answer must be given to each of these interrogatories, I am compelled to think that the ingenious hypothesis of M. De La Rive is entirely inadequate to account for the phenomenon of the Aurora Borealis, and that it is in fact inconsistent with many of the phenomena.

Some who have admitted the inadequacy of any *electrical* hypothesis hitherto advanced to account for the origin of the Aurora Borealis, have ascribed the phenomena to *magnetism*. It must be admitted that magnetism has *some connection or other* with the Aurora. The disturbance of the magnetic needle during an auroral exhibition; the effect occasionally observed on the wires of the magnetic telegraph; the relation of the auroral columns to the magnetic meridian; the formation of the corona around the pole of the dipping needle,— these facts plainly indicate the existence of such a connection. But it may still be uncertain whether magnetism holds to the Aurora the relation of cause or effect. These facts merely prove that the auroral vapor has magnetic *properties;* they prove nothing respecting its *origin*, which is the main point in question. Matter derived from the planetary spaces, as meteoric stones, have usually been, to a great extent, ferruginous, a fact which favors the idea that the nebulous matter of which we suppose the Aurora to consist is of a similar nature, and therefore magnetic.

Scarcely any hypothesis alleged to account for a phenomenon of nature is so poor as not to explain some portion of the facts. Electricity and magnetism may severally account for some of the phenomena of the Aurora Borealis; but there are other facts which neither of these causes appears to me to touch, especially the production of the material itself, or auroral vapor, or the great extent of the auroral exhibitions, or their periodicity; while I believe that each of these facts is fully and satisfactorily accounted for in my paper already referred to, which assigns to the Aurora a cosmical origin.

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4. On a New Source of Electrical Excitation. By Mrs. Eunice Foote, of Seneca Falls, N. Y.

I have ascertained that the compression or the expansion of atmospheric air produces an electrical excitation. So far as I am aware, this has not been before observed, and it seems to me to have an important bearing in the explanation of several atmospheric and electrical phenomena.

The apparatus used was an ordinary air-pump, of rather feeble power, and adapted either to compress or exhaust the air. Its receiver was a glass tube about twenty-two inches in height and three in diameter, with its ends closed by brass caps cemented to it. At the bottom was a stop-cock and a screw, by which it was attached to the air-pump. To the top were soldered two copper wires, one hanging down within the tube, terminating in one or more points, and reaching to within about six inches of the bottom, the other extending from the upper side of the cap to an ordinary electrical condenser.

In experimenting after compressing or exhausting the air within the receiver, the wire reaching to the condenser was disconnected from it, the upper plate was lifted from its place by a glass handle, and its electrical condition tested by a gold-leaf electrometer. I have found it convenient first to compress the air and close the stop-cock, when the condenser would be found to be charged with positive electricity; then after discharging all traces of it both from the condenser and the wire leading to it, the air was allowed to escape, and the condenser would become recharged to an equal extent.

My experiments with this apparatus have extended over about eight months, and I have found the action to bear a strong analogy to that of the electrical machine. In damp or warm weather little or no effect would be produced, whilst at other times, particularly in clear cold weather, the action would be so strong as to diverge the leaves of the electrometer to their utmost extent. In warm weather, when no action would be produced, I have attained the result by cooling the air artificially. A sudden expansion or contraction always increased the effect.

The results with oxygen gas were similar, but I was not successful with either hydrogen or carbonic acid gases.

It is believed that the results which have been obtained on a small scale, in my experiments, may be traced in the great operations of nature. The fluctuations of our atmosphere produce compressions and expansions sufficient to cause great electrical disturbances.

Particularly should this be observed in the dry cold regions of our atmosphere, above the effects of moisture and vapors. And it was established by the experiments of Becquerel, as well as those of Gay Lussac and Biot, that the electricity of the atmosphere increases in strength with the altitude. A manifest relation, moreover, between the electricity of the atmosphere and the oscillations of the barometer has frequently been observed. Humboldt, treating upon the subject in his Cosmos, remarks, amongst other things, that the electricity of the atmosphere, whether considered in the lower or the upper strata of the clouds, in its silent problematical diurnal course, or in the explosion of the lightning and thunder of the tempest, appears to stand in a manifold relation to the pressure of the atmosphere and its disturbances.

The tidal movements of our atmosphere produce regular and systematic compressions twice in twenty-four hours. These occur with so much regularity within the tropics, as observed by Humboldt, that the time of day is indicated, within fifteen or twenty minutes, by the state of the barometer. Saussure observed a diurnal change in the electricity of the atmosphere, corresponding with the diurnal changes of the barometer. The electricity of the atmosphere, he observes, has therefore a daily period, like the sea, increasing and decreasing twice in twenty-four hours. It, generally speaking, reaches its maximum intensity a few hours after sunrise and sunset, and descends again to its minimum before the rising and setting of that luminary.

The incessant change or oscillatory motion which we discover, as remarked by Humboldt, in all magnetic phenomena, whether in those of the inclination, declination, or intensity of these forces, according to the hours of the day and the night, and the seasons and the course of the whole year, leads us to conjecture the existence of very various and partial systems of electric currents on the surface of the earth.

The electrical excitations that accompany the aerial tides are in constant motion. They pass around the entire earth once in twenty-four hours, and encircle it with successive waves of electricity of great extent. And I have imagined, that it might be worth the inquiry, whether this did not afford a cause sufficient to explain the induced magnetism of the earth and polarity of the magnetic needle.

Several circumstances seem to favor the supposition.

1. An intimate connection has been established between magnetic intensity and the electric tension of the atmosphere.

2. All magnetic phenomena show an increase of activity during certain hours of the day, corresponding to, and obviously connected with, diurnal atmospheric changes.

3. The recent investigations of Sabine and others have shown that terrestrial magnetism is subject to an annual variation, which depends upon the relative position of the sun and the earth, being at a maximum when the latter is in perihelion, and a minimum in its aphelion, in the same way that the tides are affected by the distance of the sun.

In like manner the magnetic intensity is affected by the position of the moon, and suffers a monthly change depending upon its distance.

4. Magnetic intensity is greater when the sun and moon are in conjunction than when they are in quadrature.

5. The diurnal variations in the declination and dip of the magnetic needle accord with the position of the sun in its apparent course around the earth.

6. The maximum of these variations also changes its sign at the time of the two equinoxes. Thus whilst the deflection is eastward, up to the 21st of March, a change then occurs, and it passes to the westward with a mean equal to the eastern variations of the preceding six months.

It has also been shown, that there is a variation in magnetic declination, dependent on the change of the moon's position in relation to the meridian of the place of observation.

7. Great magnetic disturbances or storms, extending sometimes to a great distance, I have found to accord with great barometric waves passing over the same space in separate and independent systems.

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5. On the Quantitative Assay of Chromium by Blowpipe Processes. By Eugene W. Hilgard, Ph. D., of Washington, D. C.

The great practical convenience of the mouth blowpipe, and the rapidity with which, in the cases most frequently recurring, its application led to satisfactory results, could not fail to assign to it, at an early period, an important place among the means employed for qualitative researches on inorganic bodies. Nevertheless, its usefulness, even in many quantitative determinations, is comparatively a modern discovery; not so modern, indeed, as not to have had time to become generally known; and it is truly a matter of surprise to those practically conversant with those elegant methods we owe chiefly to the genius of Plattner, that these are not more generally appreciated and employed. This is the case not only in this country, but also in Europe, where the details of the quantitative blowpipe assays are unknown to many analysts of the highest standing; even there, until recently, a full and systematic course of instruction in the use of the blowpipe was hardly to be obtained outside of the Academy of Mines, at Freiberg, in Saxony, where, until a few years past, Plattner himself taught the use of his favorite instrument. Having myself enjoyed the advantage of his personal guidance in the study of this subject, it has ever since continued a favorite with me, and it is thus I have attempted to still further extend the practical usefulness of these processes — not only those involving the immediate use of the blowpipe itself, but all such as can be conveniently and advantageously carried on by the aid of that admirably contrived micro-laboratory known as "Plattner's *Blowpipe Chest*," not excluding, of course, the addition, if necessary, of such articles as do not interfere with one of the cardinal virtues of the whole, *compendiousness*. The latter is one of the main conditions of its *practical usefulness, that* being in fact the paramount consideration, to which even that of extreme accuracy must in some cases yield.

The primary object of quantitative blowpipe assays is essentially a practical one; they are, first of all, conjointly with the qualitative tests, performed in like manner, to serve as a reliable guide to those who, while engaged in preliminary researches involving the useful metals, have no laboratory at their disposal, and also, in most cases, but a limited space of time. Thus in geological field surveys and explorations of mines; also in studying the character of the products during the progress of smelting or similar operations, etc. We have here two conditions, namely, *compendiousness* and *quickness of performance*, which notably reduce the number of processes applicable in this case to the quantitative determination of any one substance. The process that fulfils them, and at the same time allows of a degree of accuracy sufficient for practical purposes, will undoubtedly be of great service to practical men. If simultaneously it allows of reaching the degree of accuracy actually obtaining in the laboratory (differing essentially from that pretended to be attained by many who attach so much importance to the third and even the fourth decimal in their percentage calculations of analyses), its value will be greatly enhanced. Several of PLATTNER's quantitative methods fulfil not only the two former, but also the latter, condition. The blowpipe assay of gold, silver, cobalt, nickel, and even copper, if carefully performed, yields results quite as accurate as those usually arrived at by a much more laborious and circumstantial route, in the wet way. With respect to that of nickel and cobalt, we may unhesitatingly prefer it to all methods of determining these metals in the wet way, except that of LIEBIG, which, however, is not frequently resorted to on account of its lengthiness, and the annoyance occasioned by the evolution of so much hydrocyanic acid.

The cobalt and nickel process was entirely PLATTNER's own invention, and we may well call it the climax of all blowpipe achievements, if we consider that by its means we can, in the space of about three hours, determine consecutively in one and the same one hundred milligrammes of ore, no less than six metals, five of which with great accuracy. The silver, gold, copper, lead, and tin processes were, in their main features, known and practised in the assayer's furnace before PLATTNER adapted them to the blowpipe, a labor which, however, for difficulty and ingenuity required in carrying it through, is quite equal in merit to a new invention. This will be readily appreciated when we recollect the main conditions to be fulfilled, as before observed.

All the assays hitherto mentioned are performed in the dry way alone, and this is one of the main causes of their expeditiousness. The operations, constantly recurring in working in the usual wet way, of evaporation and filtration (not to speak of precipitates requiring from twelve to seventy-two hours' rest before they can be filtered), consume by far the largest part of the time of the operator; whilst in the dry way operations just mentioned, the purely mechanical manipulations take up only a very small quotum, most of the time being that which is requisite for the completion of chemical action. In all these cases the substance to be weighed is obtained in the form of a regulus or bead, which rarely requires more than simple wiping or squeezing to clean it from all adhering foreign matter. But even when this is not the case, namely, when in fusions powdery precipitates are formed, which require washing on the filter, an advantage is very generally gained in consequence of the greater expeditiousness which their washing by filtration admits of, as compared with that of the large number of gelatinous, slimy, and other precipitates obtained in the wet way, which so often clog the filter and levy a heavy contribution on the patience of the operator. There seems to be at present prevailing among analytical chemists a tendency to neglect the dry and employ the wet way exclusively, which would hardly seem justified by the comparative accuracy thus obtained. When an analysis requires ten or fifteen filtrations, and perhaps as many pounds of distilled water, so often of doubtful purity, and slowly but surely dissolving even the Bohemian breaker glasses, the chances of error are undoubtedly very great, and greatly enhanced by the lengthiness of the operations. To abbreviate these, and avoid their repetition as much as possible, is an important point, which, in not a few cases, may be gained by suitably combining the dry with the wet method. There are exclusive neptunists, however, in chemistry, as well as in geology, who will boast of "doing *everything* in the wet way."

The faults contingent on the ordinary method of filtering and weighing precipitates obtained in the wet way, are completely avoided in another class of analytical operations, usually designated as the "*titration methods.*" Of late, this branch of analytical chemistry has been rapidly growing in importance; the elegant iodometric methods of BUNSEN have of themselves opened a vast field, from which further rich harvests cannot fail to be gathered in time; to the manufacturer the metric methods have already become of the highest importance. To the analyst, weary with filter and balance, when sometimes, after weeks of careful labor, he finds, in summing up his results, that according to them he has either created or annihilated a per cent. or two of matter, the burette is a real solace; through it he can recover his shaken faith, and convince himself that the third decimal may really sometimes have a meaning. The universal application of metric analysis seems indeed to be the promised land of analytical chemistry. Thus far, however, its application is still somewhat limited; and moreover, its usefulness in the field is, and probably ever will be, impaired by the bulkiness of the materials required, being mostly dilute solutions, which cannot be readily prepared in the field; and if so, the distilled water necessary would be sufficiently cumbrous to carry. When, therefore, we have to do with metals difficultly fusible or reducible, and which cannot be brought into any fusible alloy, nothing remains but to partially, at least, resort to the wet way. Such is the case with chromium, which, having at present become of considerable practical importance, I have for some time been attempting to devise a convenient method of determining it quantitatively by the aid of what is conventionally termed blowpipe processes, although all of them do not necessarily involve the use of that instrument. PLATTNER himself has proposed a mixed method of this kind for iron, involving, however, several tedious filtrations, in consequence of which it is, I believe, but rarely employed. I do not know that I have succeeded much better in the method I propose for chromium; yet it may prove of some use as enabling the explorer to execute in the field what thus far has only been done in the laboratory, and shortening the labor of those who, in manufacturing processes, have to repeat with frequency determinations of the metal in complex residues.

Nor can I claim having invented any thing essentially new; I have only attempted to adapt phenomena well understood before, to the particular purposes in view. In my description I shall proceed with reference to PLATTNER's book, and the well-known arrangements of his blowpipe chest; and I shall also follow his example with respect to minuteness in the description of the detail of the operations. In this respect some are inclined to find fault with PLATTNER, considering his minuteness as superfluous and doctrinarian. I believe, however, that all those addicted to the habitual use of the blowpipe will testify to the importance and usefulness of this particular point. In the analytical laboratory, the operations are comparatively few and simple, so that, with some general rules and experience, the analyst may, in most cases, at once successfully execute a new method, of which he only knows the rationale. With him, moreover, a few hours' longer duration of an analysis — in consequence, perhaps, of some precaution neglected, the necessity of which he could not foresee — is of much less consequence; allowing that much latitude, it may frequently be that there are several ways of doing the thing correctly. But no such latitude exists in those cases for which the blowpipe methods are specially intended; it must be done within the shortest possible space of time, and with comparatively limited means at disposal; the filtrates must not exceed in bulk what a certain very small beaker-glass will contain; and a crucible which serves very well in a fusion conducted in a certain way, will allow the mass to run over with a very slight deviation from that precise way. I have always found that I fared best in following to the letter those seemingly pedantic rules of Plattner; and whenever I deviated from them, I found myself almost unconsciously returning to them after a short excursion. They are the results of thorough practical experience of a head as thoroughly practical.

I shall therefore describe minutely that method of manipulation which I have thus far found to answer best. But my experience is short; and as moreover I cannot lay claim to the high qualities of my illustrious teacher, there is room left for a great deal of improvement.

The facility with which chromium may be separated from most other bases, by conversion into chromic acid, by means of fusion with alkalies and simultaneous action of oxidizing agents, immediately suggests this as the first step in the assay.

In the qualitative test, this fusion is effected in the large platinum spoon, by means of the blowpipe flame itself. The great fluidity of the flux, and the strong evolution of gas accompanying this process, render a considerable loss by spirting unavoidable, unless the vessel in which the fusion takes place is covered. In this case, however, the free access of atmospheric oxygen is cut off, and it is necessary to supply this want by the addition of an oxidizing flux. As such, chlorate of potash may occasionally be used; but the *nitrate* is by far the most convenient, because admitting of a higher temperature; besides, it is already a necessary inmate of the re-agent case.

Even when vessels as large as the large porcelain blowpipe dishes are used, loss by spirting occurs. I tried to render the flux less fluid, or more tenacious, so that in bursting, the gas bubbles would not throw up any of the mass. Borax will not do this, but a very slight addition of phosphate of soda (a crystal of microcosmic salt fused) diminishes the spirting very much; yet it does not entirely prevent it.

The common clay crucibles covered with a torrefaction dish, will not, of course, answer, on account of their porosity. Porcelain crucibles are so much attacked by the flux as to be useless after two or three short fusions, and would thus, in the end, turn out very expensive. I therefore resorted to a platinum crucible of precisely the pattern of the clay ones, with a platinum torrefaction dish for a cover.

With these, the fusion may easily be executed without the least loss, the cover being put on with the convex side downwards. It is but slightly out of shape on one side, so as to allow an opening for the escape of gas. This is essential; for if it fits pretty close all around, and the fused mass inside happens to rise near to the edge, the whole seam is instantly closed gas-tight all around, in consequence of capillary attraction, and immediately after the cover is forcibly lifted off by puffs, each of which ejects a few globules of flux. This orifice, therefore, must always be kept very hot during the fusion, to prevent its being closed up by some of the fluid mass solidifying around it.

The use of the hydrate of potash, which of all decomposes quickest the chromic iron ore, involves several inconveniences. The commercial article always contains more than its water of hydration, in consequence of which it fuses at a low temperature, and then boils very noisily in losing its excess of water. Moreover, it attacks the platinum so powerfully in a lengthy fusion, that no crucible of ordinary thickness could survive a great many of these.

The carbonates of potash and soda are much more manageable; and there are cases in which they cannot well be dispensed with, as will appear hereafter. They do not sensibly corrode the platinum, but their strong effervescence with some ores is somewhat troublesome.

In most cases I now confine myself to the use of nitre alone, which I find to answer the purpose perfectly. The anhydrous potash formed by its decomposition at a high temperature, has very little effect on the platinum, — incomparably less than the hydrate, and the action on the ore is nearly or quite as rapid as in presence of the carbonates.

When after this fusion, the mass is lixiviated with water, most of the bases remain either in the free state or in combinations insoluble in an alkaline solution.

When the alkaline earths are present, (which in a fusion with nitre would in many cases remain in the caustic state,) the chromic acid will be rapidly withdrawn from the alkaline solution to form insoluble chromates; it is necessary, therefore, to combine those bases with some other acid forming an insoluble salt with them. In this case some carbonate of potash or soda may be used together with the nitre, since the carbonates thus formed remain unaltered in an alkaline solution of chromic acid.

When lead, tin, bismuth, or cadmium is present, the complete separation of the chromic acid is more difficult. Chromate of lead may indeed be decomposed by fusion with nitre or carbonated alkalies; but as soon as water is added to the fused mass, the reverse decomposition takes place, the lead assuming the chromic acid, and leaving carbonate of alkali in solution. Even the sulphate of lead is converted into chromate by contact with a solution of alkaline chromates. With the other metals mentioned, the case is similar.

The separation may, however, be successfully effected, when in the fusion opportunity is given to the oxides of forming silicates. When, therefore, as in most cases, the ore itself does not contain the requisite amount of silica, the latter must be added in the flux. When chromate of lead is intimately mixed with an excess of finely divided silica, either precipitated, or, better still, such as is obtained from fluosilicic gas, the decomposition, in the fusion with nitre, is complete; the silicate of lead formed does not retain a trace of chromic acid, when washed with water. Tin and bismuth behave in precisely the same way; I have not tried cadmium, but it can be foreseen that it would act similarly. It will be observed, that the introduction of silica into the flux can also be applied to the case of the alkaline earths, which, in some cases, might be preferable to the use of the carbonates. A basic silicate of potash, of course, remains in the alkaline solution; and with it most of the acids that may have been present in the ore, as such, or formed by fusion, namely, chromic, vanadic, manganic, molybdic, tungstic, titanic, tantalic, niobec, antimonic, arsenic, phosphoric, and some of the other non-metallic acids. As for the silicic acid, it is easily got rid of by adding to the solution, before filtering, nitrate or carbonate of ammonia. The manganic acid is, at the same time, thrown down by the addition of some alcohol — not acetic acid — since that might dissolve some of the bases, and would, besides, unnecessarily neutralize the solution. After filtering the solution, it remains to bring the chromium into some weighable shape, separating it at the same time from the foreign substances still in solution. When none of the fixed acids are present besides the chromic, the latter might be at once precipitated by proto-nitrate of mercury in the neutral solution. The condition but just mentioned would alone restrict the use of this method to a small number of cases; but however convenient and accurate, when several hours can be allowed for the separation of the precipitate, the results obtained fall short of the truth seriously, when, as in the field, no such delay is admissible. In practice, therefore, we should probably dispense with its employment altogether. When fixed acids are in solution, a simple method of separation suggests itself, in the evaporation to dryness and fusion of the residue with a mixture of carbonated alkalies and cyanide of potassium. Most of the acids thus enter into combination with the alkali, while the chromic acid is reduced to the sesquioxide, and might, it seems, be obtained pure by filtration. Unfortunately, however, the chromic oxide thus obtained always contains a considerable amount of potash, which it will not part with on any consideration short of a fusion with nitre; the quantity of potash thus contained is, moreover, quite indefinite and variable. One more filtration being about the greatest allowance still to be made if the process was to be useful in the field, I had to reject several other methods on this consideration. Turning to the extreme opposite of my first plan, I sought for a compound of chromium that should be insoluble in an *acid* solution of the accompanying acids. Among these, the insoluble modification of chrome alum seemed to answer best, and I have finally based the determination of the metal upon the formation of this compound. It is well known that when solution of chrome alum, rendered acid by oil of vitriol, is heated to 200° centigrade, a light green powder is precipitated, the formula of which is also, KO,

$SO_3 + Cr_2O_3, 8\,So_3$ — ashydrous chrome alum. This singular substance is almost entirely inert; it is perfectly insoluble in water, is not decomposed by boiling acids, but may be slowly and imperfectly decomposed by long boiling with potash ley. Even nitre attacks it with more difficulty than it does the pure oxide. By ignition, it is converted into a mixture of chromic oxide and sulphate of potash, in the same atonic proportions as in the original salt. The transformation of the chrome alum into this salt is not, however, complete at 200°; it is only an indefinite quotum of the whole which thus passes into the insoluble state. When the solution is further heated, until the oil of vitriol begins to volatilise, the conversion is complete; and upon dissolving the mass in water, traces only of chromium remain in solution, while a light green powder falls to the bottom. This has the same composition with the salt before mentioned, but it differs in being decomposable by hot water, which gradually resolves it into sulphate of potash and insoluble tersulphate of chromic oxide. The decomposition is slow, and requires a long time for completion; yet it begins very soon when the temperature is raised. At ordinary temperature the salt is not changed by water. I have not had leisure to decide experimentally whether soda and ammonia form a similar salt with tersulphate of chromium. Their presence does not influence the result when an excess of potash salt is present.

The formation of the salt, as far as my experience goes, takes place quite independently of other acid substances present, so long as there is an excess of sulphuric acid or of bisulphate of potash; so that all acids soluble in an acid solution of this kind, (to which other solvents not otherwise interfering might, if necessary, be added,) may be separated from the chrome salt by mere washing.

The washing of this salt on the filter presents some difficulties. So long as an abundance of other salts is held in solution by the liquid, the precipitate remains powdery and does not clog the filter; but as soon as the fluid becomes very dilute, the powder diffuses and begins to pass through the filter, which very soon becomes most effectually clogged, putting an end to all filtration.

Washing with a saturated solution of sal ammoniac prevents the diffusion and expedites the washing; yet it cannot thus be done in less than three or four hours. The process becomes more expeditious when, instead of immediately throwing the precipitate of the filter, it is washed by the method of partial decantation, proposed by Rose; which so essentially facilitates the washing, otherwise so tedious, of such precipitates as alumina, etc. The powdery chrome salt settles down pretty quickly into so small a volume, that with even the small beaker glasses of the blowpipe chest, a coefficient of progression as high as 25 or 30 is easily obtained; so that after four or five decantations, the washing is complete, and the precipitate may be thrown on the filter, dried and ignited.

Withal, this is sometimes a very slow process. I have occasionally succeeded in thus washing the precipitate in 35–40 minutes; but sometimes, from causes not always controllable, it requires a much longer time to settle; or if the liquid be filtered while turbid, the filter becomes clogged, and then, in consequence of the large bulk to be filtered, the operation becomes interminable. At the same time the quantity of sal ammoniac necessary to form so much of a saturated solution, is of quite unblowpipelike proportions. I could find no other salt to answer better, or even as well, as the chloride of ammonium.

I therefore attempted to involve the powdery precipitate of the chrome salt in another one, which should settle quickly and take the other with it; being, at the same time, of such a nature as not to interfere with the separations, and completely volatile in the ignition. The method applied by Knapp and Arendt to the washing of the ammonia phospate of uranium, (shaking with a few drops of chloroform,) however well it answers for bulky precipitates, seems to be inapplicable to powdery ones. Sulphuret of carbon did no better than the chloroform; nor any organic precipitate like gum arabic or glue by alcohol or tannin, and others I tried.

Resorting to the salts of mercury, (most of the persalts being soluble in an acid solution,) I tried first the iodide, which answers pretty well, but has the inconvenience of being required to be precipitated in considerable quantity. The chloro-sulphide, such as is precipitated from a solution containing an excess of perchloride by a dilute solution of sulphuret of ammonium, answers better; and but a very small amount need be thrown down to clear the liquid perfectly, when the washing may be done either on the filter or by decantation, in the space of 35–45 minutes.

It might be objected, that in case of the solution containing antimonic oxide, arsenic, or molybdic acids, precipitable by HS, a precipitate of the sulphurets of these metals might be formed, the two latter of which would subsequently increase the weight of the residue. Such a precipitate may indeed be locally formed; but being acted upon by a solution of perchloride of mercury, an exchange of elements would take place, by which the chlorides of those metals would pass into solution, and still chlorosulphide of mercury would ultimately be formed. In most cases, the flocculent olive-green precipitate thus formed settles quickly, leaving a clear supernatent liquid. The presence of some substances, especially antimony, has a tendency to prevent clear settling.

After washing, which must be done with a solution of corrosive sublimate, the filter is dried and ignited. It is incinerated with great difficulty, like filters containing the ammonia-phospate of magnesia — 100 of ignited residue correspond to 47.3 of chromic oxide.

When ignition is continued too long, and with free access of air, at a high temperature, chromic acid is formed, which unites with a part of the potash present, expelling the sulphuric acid. If the ignition is continued before the glass-blower's lamp, at a strong yellow heat, the precipitate keeps losing weight steadily, and a considerable portion may thus be converted into the chromate.

It is best, therefore, before weighing, to moisten the ignited residue with a drop of sulphuret of ammonium, then drying and gently reigniting it. Any chromic acid formed is thus reduced, and the potash reconverted into sulphate.

I shall now describe specially the mode of operating, with the modifications necessary when certain of the metals or acids are present.

A. Fusion with Nitre.

A. When neither Lead, Bismuth, Tin, nor Cadmium, is present in the Substance under Examination. — Such is the case among the minerals, with chromic iron ore and chrome ochre. Among the artificial products, the commercial chrome green frequently comes under this head; but it is also very frequently mixed with foreign substances of various kinds.

The chromic iron ore must be reduced to the finest division possible, otherwise the decomposition remains incomplete, or the fusion is necessarily very lengthy. The finest of the powder of the pure mineral may be obtained by elutriation, by means of two beaker glasses. Or else, the ore having been reduced to considerable fineness in the agate mortar, it may be sifted through fine gauze tied over a beaker glass, so as to form a shallow bag, over which paper is subsequently tied, so that, in shaking or striking the glass against the palm of the hand, the powder is retained, and its finest parts, passing through the gauze, are collected in the beaker-glass. But in mixed ores, formed of substances differing much in their physical properties, neither of these processes is applicable; and nothing remains but to reduce them to the proper state of comminution in the mortar.

Chrome ochre and artificial chrome green are oxidized with violence by the nitre, and need not be so carefully comminuted.

Having weighed off 100 milligrammes of the chromic iron ore, or from 50 to 75 of the chrome ochre or chrome green, the substance is poured into the small platinum crucible, (which, like the clay ones, is usually supported on the smaller cupel iron,) and the latter placed on the triangle of the iron ring of PLATTNER's lamp. In the case of the chromic iron ore, about three large spoonfuls of nitre are added at once; the crucible is then covered with the platinum torrefaction dish, in such a manner that the orifice left for the escape of the gas is sideways; in order that by a simple movement of the support round its pivot, this orifice may be approached to, or removed from, the flame. The spirit lamp is then provided with the iron chimney, which in this case is best set on the lamp itself; for which purpose it must have three feet, about five millimeters high, attached at its base, so as to admit a current of air. The flame is so adjusted that the point of the luminous cone is slightly truncated by the bottom end of the crucible.

The heat thus produced is fully sufficient when nitre alone is employed; if raised higher, so that the upper rim of the crucible becomes very hot, the nitre ascends by capillary attraction, and then spreads over the outside of the crucible, until it reaches the triangle wires; in which case, if these are iron, the process must be abandoned. By using a triangle of platinum, this danger would be avoided, since *that* might subsequently be washed off like the crucible itself. Such a triangle would be particularly useful in long fusions, where an efflorescence of the nitre on the outside is unavoidable; although, unless the heat is unnecessarily raised, the difficulty may easily be avoided in most cases. The lamp being placed on the circumference of the circle in which the support moves round its pivot, the crucible is moved over the flame and left until the effervescence of the nitre can be distinctly heard, when it is quickly moved to one side, so as to allow the flame to play mainly on the orifice side of the crucible. In this way it is very easy to regulate the action in such a manner that no loss may accrue from excessive effervescence. Even if the molten mass should rise so as to touch the cover, it cannot lift the latter off so long as the orifice is sufficiently large and kept very hot.

In chromic iron ores, the effervescence generally ceases very soon; the crucible may then be moved vertically above the flame, which may also be raised a little, and the whole left quietly for about twenty-five or thirty minutes, at the end of which, if the ore has been sufficiently comminuted, the decomposition is complete; the crucible is removed from over the flame and left to cool. The oxidation of the chrome green is so rapid and violent, that if the whole of the nitre were added at once, loss would be unavoidable. To fifty milligrammes of this substance, therefore, only about half a spoonful of nitre must be added at first; the whole is then heated with the precautions before described, the nitre being added by half spoonfuls, and no sooner than the effervescence caused by its predecessor has completely subsided. At each of these interruptions, the cover is lifted off while the mass is fluid, by means of the pincers, which must be quite clean at the points, so that if any thing should adhere to them they may subsequently be washed off into the principal solution. Great care must be taken not to lose any thing from the cover, to the lower surface of which a large drop is generally adhering towards the end of the fusion; it must not therefore be laid down, but kept in hand; the mass remaining in the crucible is allowed to cool until the surface solidifies, before the nitre is added. If dropped into the melted mass, small particles are frequently thrown out of the crucible. When, on account of the presence of alkaline earths, carbonate of soda or potash requires to be added (a spoonful being about as much as is ever required), it may be thrown into the crucible at once, with the first dose of nitre. Such fusions require a higher temperature than when nitre alone is used, and there is less danger of loss by efflorescence. A little experience easily enables one in most cases to judge when enough nitre has been added, and the oxidation is complete. In the chromic iron ore, no criterion for the completion of the process is available, save subsequently testing the washed residue for chromium.

b. *When lead, bismuth, tin, or cadmium (either or all of these metals), is present in the ore.*—To this class belong the yellow-lead ore, vanqulinite, and a few rare minerals; besides, mixed ores containing these; and of artificial products, chrome red and chrome yellow; chrome green also sometimes is mixed with carbonate of lead. 100 milligrammes of such substances having been weighed off, are transferred to the agate mortar, and there intimately mixed with fine silica, of which an excess must be used, involving different quantities according to the state of division it is in. Of silica obtained in the preparations of hydrofluosilicic acid, three to four spoonfuls, as full as may be, are requisite; it loses its voluminosity in mixing with the ore. Of silica, such as is usually obtained in analysis, about the same nominal amount, or a little less, may be used,—its spoonfuls being only about half the volume of those of the former; at the same time, not being so finely divided, it does not act so readily, and proportionally more is required. After mixing the ore thoroughly with the silica, two heaped spoonfuls of nitre are added and triturated with the mixture. This serves to remove or loosen most of what has adhered to the mortar and pestle. The crucible is then set on the triangle, a spoonful of the mixture thrown in, and the process conducted as before described, adding a spoonful of the mixture at a time. When all has been removed from the mortar as far as the spoon will do it, half a spoonful of nitre is thrown into the mortar and triturated, removing, as much as possible, all that still adheres. The nitre is then transferred to the crucible, and the same operation performed with three more half spoonfuls. The mortar is thus effectually rinsed, and all the ore removed. When the decomposition is complete, the fluid mass may be observed to be perfectly clear in the centre, while to the sides and bottom of the crucible flakes of silicate are adhering. During the cooling it is generally easy to see whether or not the chromate has been perfectly decomposed, for while the flakes of the silicate appear white or yellowish, and evidently colored by the flux alone, undecomposed particles of metallic chromate will appear dark red, and pass into orange yellow on cooling. When, in consequence of incomplete mixture or an inadequate supply of silica, such particles are still to be seen in the mass, no further addition of silica will remedy the evil, and the process has to be repeated with another quantity of substance.

After such fusions, the sides of the crucible frequently appear blackened, but this does not seem to injure them. In most cases, ignition alone, and in all, fused bisulphate of potash, will remove the discoloration.

Most of these fusions, always excepting that of the chromic iron ore, can be made in the space of from five to ten minutes. When performed at a high temperature, it sometimes happens that much of the nitre is volatilized, and the mass remains almost dry. When this happens it is always best to add another spoonful, lest the decomposition might have remained incomplete.

B. Further Treatment of the Melted Mass.

In but a few cases the fused mass will dissolve in water without leaving an insoluble residue, from which it must be separated by filtration. If manganese or silica were present, they will in part have passed into solution, and must be removed before filtration. The manganate of potash is easily removed by the addition of a few drops of alcohol; in most cases it would be destroyed, and the manganese retained, by the paper of the filter. When, as in the majority of cases, perhaps, there was silica present in the ore, a part of it may usually be supposed to pass into the aqueous solution, from which it is best removed by either nitrate or carbonate of ammonia.

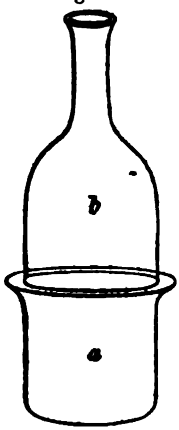
The former rather deserves the preference over the other, for completeness of precipitation. But a solution in which SiO_3 has been precipitated by AmO, No_5 will not filter well unless previously either shaken or boiled; stirring alone will not readily induce the SiO_3 to collect in flakes. When there is any objection to heating the solution, as when antimony was present in the ore, the crucible and cover, after having cooled down sufficiently, are transferred by means of the pincers, to a small beaker containing about one quarter inch high of water, and is there left until the mass is dissolved; the solution is then poured off into a test tube, of which it fills about two thirds; it is not necessary to rinse the beaker, since what little remains in it will not afterwards interfere with the filtration. A spoonful of nitrate of ammonia is then added, and closing the test tube with the thumb, it is well shaken. Subsequently the solution is poured back into the beaker, and test tube and thumb well rinsed. When, as in most cases, the boiling of the solution is admissible, the solution of the melted mass may be accelerated, while, at the same time, the silica is more safely precipitated, by boiling it, crucible and all, in the same vessel which subsequently serves for evaporation, and which will be presently described.

The boiling need not be continued longer than half a minute, after which the solution is poured over into a beaker, the crucible and cover being likewise transferred by means of the pincette. The evaporating vessel and its cover are rinsed as economically as possible, and set up for evaporation; which is to be carried on simultaneously with the filtration.

The filter used ought not to be too small, otherwise the operation becomes too lengthy. When, in washing a precipitate, the rapidity of filtration of the fluid falls short of a certain ratio, it becomes disadvantageous to wait for the draining of the last few drops, which, in consequence of the decrease of hydrostatic pressure, may require as much time as the nineteen twentieths which have preceded them. The same, of course, applies to small filters. A two inch filter is about the average size I have found most advantageous in present instance.

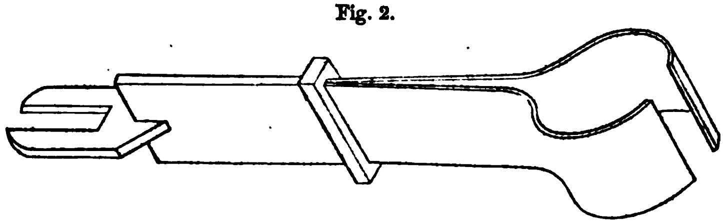
So soon as the principal solution has passed through the filter, it is transferred to the evaporating vessel, which I must say a few words about. The evaporation must be done in the same vessel in which, subsequently, the fusion with bisulphate of potash is to be effected. A common porcelain blowpipe dish answers the purpose imperfectly; it is generally too small; and no *one* vessel will resist many fusions of the bisulphate in it; besides, it requires a long time to cool. But, above all, it has the inconvenience of obliging the evaporation to go on very slowly, for fear of loss by spirting. The evaporation may be performed very safely and expeditiously in a little glass flask, with a neck about two and a half inches long; the fluid may then be kept in a state of violent ebullition without fear of loss, if the flask is not much above half full. It is not easy, however, to find a glass that resists repeated evaporations and bisulphate fusions, and thus the experiment is frequently lost. I find that all these objections may be obviated by using as an evaporator a platinum dish, of the same form as the porcelain ones, but somewhat larger and higher. On this is set a small bell glass, shaped like the upper half of a champagne bottle, open at the top, and of such a diameter at its base as to fit the platinum dish just at the point where the curvature of the rim begins, as shown in fig. 1, which represents the whole in half size. The dish being set on the grate, placed at the proper height for the spirit-lamp flame, the little bell glass cover is clamped by its neck into a holder (fig. 2, full size), which in its turn is clamped into the arm which, at the same time, carries the filtering support; both must, therefore, be put on before the filtration begins.

Fig. 1.



In this evaporator, which is useful in a great many other operations, the fluid may be kept boiling violently all the time, even when the dish is filled up to nearly where the glass is set on. The seam where the two meet is closed up with fluid by capillary attraction, and there is no danger whatsoever of any loss occurring there, unless the lamp is so placed as to heat the rim itself and make the fluid boil there. Nor does it matter if the froth inside rises up to the neck of the glass

Fig. 2.



cover; the whole may in fact be considered as a close flask. The evaporation usually goes ahead of the filtration; from time to time, as the filtrate accumulates, it is poured into the evaporator from above, the lamp having for a moment been removed. The fluid ought to be poured into the middle, so as not to touch the glass.

When an alkaline solution of chromate of potash and nitre is evaporated in this manner, some of the salt usually effloresces on the rim, causing inconvenience, and liability to loss. To prevent this it is best to add the sulphuric acid and bisulphate of potash before the evaporation begins. The oil of vitriol is added, drop after drop, until the yellow color of the solution changes to orange, and after that, four or five drops more. The amount of *bisulphate* to be added varies according to the amount of nitre that has been previously used; three to six or seven heaped spoonfuls are the average — an excess does no harm.

The last two drachms, or thereabouts, of the filtrate are reserved until the fluid in the evaporator has been reduced nearly to the point when the oil of vitriol begins to volatilize, and heavy fumes of nitric acid are seen to pass off. The bell cover is then removed by raising the arm that carries it so far as to allow of moving it sideways from the dish. It is then unclamped and rinsed into the dish by means of the remaining part of the filtrate, first on the outside of the lower extremity, then, being inverted, on the inside, like a funnel; finally, a few drops of distilled water from the dropping bottle finish the process. It is well also to wash down what may have effloresced on the rim of the basin; all these washings ought not to fill the latter more than one third full. In order to perform the rest of the evaporation and the subsequent fusion quickly and safely, the basin is now covered with a platinum cover of watch-glass shape, which must be smaller than the outer circumference of the rim of the basin. The boiling process is then resumed, the flame being somewhat lowered so as not to make the ebullition too violent; there is generally very little danger of any loss, unless the cover fits *too tight*. After some time, a frothing noise commences inside, announcing that the last of the water is escaping, and shortly after sulphuric acid fumes appear. At this period the mass inside forms a beautifully green syrupy liquid, which, on cooling, crystallizes; but on being further heated, it suddenly turns of a peach-blossom color, and becomes turbid with the double salt of chrome and potash. The heating is continued until *dense fumes* cease to be evolved, when only fused bisulphate of potash remains. The lamp having been removed, the cover is rapidly lifted off with the pincers, taking care not to lose any of the fluid mass which may be adhering to its lower surface; being allowed to cool an instant, it is laid inverted on the bottom of one of the porcelain dishes turned upside down. The basin is then seized by the rim, and, while the salt is still fluid, it is inclined and turned round and round, so as to spread the mass as much as possible. After it has solidified, the basin being replaced on the grate, six to eight drops of oil of vitriol are added, the cover is put on, and the vessel again heated until dense fumes of the acid rush out from under the lid. Removing the fire, the operation before described is repeated. The mass will now be found to remain fluid much longer; it may be rapidly cooled by dipping the bottom of the vessel in water, always taking care to spread the mass as much as possible. When perfectly cooled, it is of a light green color; if any peach-blossom colored particles are observed, it is a proof that too little potash has been used; more must then be added, together with some drops of sulphuric acid, and the whole reheated until the SO_3 is driven off; after which the mass is softened as before. It ought to remain soft enough to be readily dug up with a spatula or glass rod. If quite hard, it would take a long time to dissolve it in water, since it must not be heated; if too soft or quite fluid, so much heat would be evolved in dissolving it that the composition of the precipitate might be changed. In either case, therefore, the mass must be reheated, either to volatilize the excess of acid, or, having added a few drops more, to incorporate these with the rest of the mass. Not unfrequently a tough, green syrup is thus obtained, which very readily dissolves; at others, the mass is soft, but solid and crystalline. Next, the *cover* being taken up with the pincers, it is rinsed off with the washing bottle into a beaker glass. What part of the mass adheres to it is generally very soft and dissolves easily, containing as it does an excess of acid. So much water may be used for this purpose as will half fill the beaker; for to the *main portion* of the mass so much water must be added at once as to prevent entirely its getting sensibly heated, and for the same reason it is advantageous to scratch up and divide the mass as much as possible. A small platinum spatula, made of a piece of thick wire, is of great service in this and many other processes; it also injures the platinum basin less by scratching than a glass rod.

When all is dissolved (which, by aid of stirring and scraping, may generally be effected in the course of a minute), the solution, with the precipitate, is poured back into the beaker. Should any undissolved portions remain in the dish, they are loosened and then washed off by some of the solution, which is poured back for the purpose. A little more water is subsequently poured into the platinum dish, and the index finger of one hand (*clean*, of course,) is used to rub off from the bottom and sides such parts of precipitate as still adhere. I must here observe, that, unless the inner surface be kept very clean and smooth, some of the chrome salt may remain adhering so firmly as to resist all the rubbing that can be applied; it must be removed by fusion with saltpetre. As for the index finger, I know nothing to supply its place in cases of obstinate precipitates, and it is generally much less liable to occasion loss than a feather. The turbid fluid is then poured over into the beaker. Further rinsing may be deferred until the first liquids have passed through the filter.

About one third, or even less, of an even spoonful of powdered Hg Cl is now added, and rapidly dissolved by stirring; after which, some very dilute Am S_2 is added, drop by drop, taking time between each to stir violently for a little and observe whether or not the fluid is clear above the flakes of chlorosulphide of mercury, which collect and encase the powdery chrome salt. The clearing generally takes place when the precipitate becomes of an olive tint, when but a slight excess of Hg Cl remains. Great care must be taken to have all the precipitate well stirred up, for if a little of the chrome salt happens to remain at the bottom, it afterwards diffuses itself and gives trouble.

The flakes of the chlorosulphide settle almost instantly, and the filtration may be begun at once. I generally use two-and-a-half-inch filters, either of Swedish, or other filtering paper which has been extracted with acid, so as to give as little ashes as possible, and sufficiently constant in the same paper. In filters as small as these, advantage may be taken of the circumstance that the filtration is considerably accelerated when the filter does not fit closely to the funnel. I use small funnels made before the lamp, inclosing an angle of only about fifty to fifty-five degrees. When a regularly made filter is put into one of these, the upper edge may be adjusted closely, as it always ought to be; but its sides will gradually diverge from those of the funnel, and thus the greater part of it will hang freely inside; the weight of fluid being too small to break down the paper.

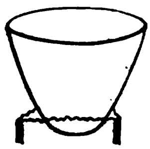
When the first solution has passed through, the precipitate is generally best thrown on the filter and washed with solution of sublimate, a small dose of which is put on the filter each time before the water is poured on, not forgetting, of course, to rinse both the platinum dish and the beaker. This is always best done with the washing bottle, using a powerful stream, so as to stir up the precipitate as much as possible. In this way the washing may generally be finished in from thirty to forty-five minutes, its completion being announced by either the evaporation test, or by a small granule of chloride of barium, added to a portion of the filtrate collected in a test tube not producing any more turbidity.

It is desirable that the precipitate should be spread over the surface of the filter as much as possible, and not collected in the lower part of it. When the last of the water has drained off, the filter being raised with the pincers on its tripled side, is carefully taken out and laid on the one half of a piece of blotting (or filtering) paper doubled several times. The paper is then doubled over the filter and pressed between the fingers, gently at first, so as not to squeeze out any of the precipitate. When dried as far as feasible in this way, it is doubled up in the usual manner, so as to form a small square package, which is further pressed between the paper, so as not only to lose most of its water, but also to remain firmly doubled. In view of this, it is well to avoid the lower end of the filter being outside; usually containing, as it does, most of the precipitate, it easily bursts open, and endangers the experiment.

The filter is now pressed into the platinum torrefaction dish; it may be carbonized, and most of the mercury volatilized over the spirit-lamp flame, being set outside into some place where the fumes can escape without molesting the operator. This done, a charcoal furnace is prepared in the same manner as when a copper ore is to be roasted, and the dish being set on the ring, the whole is heated by the blowpipe flame, with which it is easy to create such a draught of air as to quickly burn the filter, especially when, after incinerating the upper side, it is turned and broken up by means of a spatula, the vessel being, of course, taken out, and set on a piece of white paper to avoid loss.

When the incineration is complete, the torrefaction dish being removed, the furnace door is closed, and a charcoal furnace cover prepared. The incinerated mass is then transferred from the torrefaction dish (in which it lies quite loosely) to the platinum crucible, previously counterpoised on the balance together with a small platinum wire tripod, which serves to keep it upright on the scales (fig. 3, full size). It is advantageous to have a very thin crucible expressly for this purpose, so as to avoid weighting the balance too much; for in order to resist the *fusions*, the crucibles used for these must have some thickness of metal. The crucible, being set on the ring, is covered with the dish, the furnace cover put on, and as high a temperature as possible kept up for five to six minutes. After this, the precipitate will appear of a full green color all over, except

Fig. 3.



some parts which may be semifused. A piece of carbonate of ammonia may be put in, and the heating resumed until it is volatilized; but if a full heat has been given, so as to make the flame dart out at the chimney hole, there is no need of this precaution. The crucible, being taken out, is set on the triangle of the lamp, and a drop of concentrated Am. S_5 (sulphuret of ammonium) is left to fall on the porous mass, which instantly absorbs it. It is then cautiously dried over the spirit-lamp flame, and finally ignited, so as to drive off all the sulphur. If any chromate had been previously formed in the ignition, it has been thus destroyed, and the weight of the substance thus treated, deducting the filter ashes, may at once be taken as correct.

One hundred of ignited residue correspond to 47.3 of chromic oxide. The result may be verified by washing the residue on the filter until the sulphate of potash is dissolved, when the chromic oxide may be ignited and weighed. After deducting the filter ashes, the result will be found to coincide very nearly with that deduced from calculation; generally a slight excess is thus found. In repeating seven or eight times the determination of twenty-five millegrammes of chromic oxide, mixed with various other substances, and varying the circumstances a good deal, the results never varied more than 0.1 of a millegramme. In reading the detailed and circumstantial description, it may appear to some that the process itself must be lengthy and tedious. In practice, however, if all the precautions be observed, a determination of chromium may, in most cases actually occurring, be performed in the course of one and a half to two hours.

It only remains for me to mention a few precautions, necessary in particular cases; some of which, it is true, hardly ever occur in practice. It is sometimes the case that in the first filtration, the filtrate comes through turbid after the solution has become dilute. When this happens with substances containing tin or bismuth, it is a sign that a sufficient amount of silica has not been added in the fusion. It always happens when antimony is present, and may be avoided by throwing some nitre on the filter.

In general, when any of the metallic acids are present, it is advisable to render the sulphate solution more strongly acid, by adding a few drops of oil of vitriol, after solution has taken place. It is also best to wash somewhat longer, especially in the case of phosphoric acid.

Phosphoric, *arsenic*, *boric*, and *molybdic* acids require no precautions beyond those just mentioned. The same is probably true of the *titanic*, which I have not experimented on, for want of material.

Tungstic Acid. When this acid is present, it is precipitated from the acid solution, and causes violent bumping during the evaporation. It does not interfere with the formation of the chromopotassic sulphate. On dissolving the fused bisulphate mass, the tungstic acid prevents entirely the diffusion of the precipitate, which appears flocculent, and settles easily, so as to render superfluous the use of the mercurial salt. After the main solution has passed through, the filter is washed with water once or twice; six or eight drops of strong ammonia are then successively dropped on the filter; and after waiting a few minutes the filter is filled up with solution of sal ammonia. Subsequently the washing is completed with the same solution, (which need not be saturated,) to which some drops of caustic ammonia have been added.

The tungstic acid is thus completely removed, and an accurate result obtained.

Antimonic acid. After the fusion with nitre, the solution must be effected without boiling, and after addition of nitrate of ammonia, the liquid must be shaken in a test tube, to make the silica collect. The filter must be washed with solution of nitre.

After the bisulphate fusion, a spoonful or two of *tartaric acid* are thrown into the beaker before rinsing into it the cover of the platinum dish. The solution is kept very acid, and plenty of corrosive sublimate used in washing.

I have not experimented on *selenic* and *telluric* acids; but it is probable they would cause no difficulty. *Tantalic* and *niobic* acids would seem to require a third filtration; however, they are not likely to give much trouble by their frequent occurrence.

Vanadic acid I have not had an opportunity of investigating, but it would seem that it would also be separated, in the ordinary course of the operation. If present, the larger part of it would be removed in combination with ammonia, while precipitating the silica from the nitre solution.

When *magnesia* is present, which after fusion with nitre would be left in the pure or carbonated state, the nitrate of ammonia may dissolve part of it in boiling the fluid. In most cases this does not essentially interfere, since the sulphate of magnesia is soluble; still, when a great deal of this earth is contained in the ore, it is best to add silica, as in the fusion of ores containing lead.

If the ore contained *copper*, and no silica was added, yellow flakes of chromate of copper appear when the mass is dissolved. In this case a little caustic potash is added, and the solution boiled. The yellow flakes then contract into black scales of oxide, which retain no chromic acid.

The presence of *iodine*, *bromine*, *chlorine*, or *fluorine* in the ore, would interfere, inasmuch as the platinum evaporator would be attacked by them. When the chromic *oxide* alone is present, the halogens may be expelled by a fusion of the ore with a small quantity of bisulphate of potash, previous to that with nitre, and executed in the same vessel. This cannot be done when chromic *acid* is present, since then the volatile oxychloride, fluoride, etc. would be formed. I have not devised any ready general method of separation in this case, which, however, is of rare occurrence. In the presence of lead or other reducible metals, the chromic acid cannot easily be reduced by oxalate of potash, without endangering the platinum crucible; in many cases, however, the difficulty might be overcome by the use of that salt, previous to the bisulphate and nitre.

IV. PHYSICS OF THE GLOBE.

1. THE INFLUENCE OF THE GULF STREAM UPON THE SUMMER CLIMATE OF THE ATLANTIC COAST. By DR. JAMES WYNNE, of New York.

THE object of this paper is strictly hygienic. The medical practitioner is constantly called upon to give an opinion as to the adaptitude of summer resorts for his patients in such a state of health as to require a change of air or climate. To those who resort to the seaside, as, indeed, to those who go inland, the proper or improper selection of the locality may be a matter in which life and death are held in the balance; and any light thrown upon this subject will, I am well assured, be kindly received by the medical profession, and promptly acted on by their patients.

The recent examinations conducted by the officers under the direction of the Superintendent of the Coast Survey have shown that the bottom of the Atlantic Ocean presents a range of mountains pursuing a similar course to the Appalachian chain, some distance back from the coast. These examinations have not yet been completed; but so far as they have been prosecuted, they clearly demonstrate the continuity of this chain of submarine mountains, and its general course.

The effect of this configuration of the bottom of the ocean over the temperature of the water is clear and unequivocal. The whole extent of this influence has not yet been determined, but thus far it shows that the Gulf Stream is not one uniform mass of warm water, pursuing a north-easterly direction along the coast, at a pretty uniform distance from it; but a series of bands of warm water, interspersed with colder ones. These have been laid down by Professor Bache with precision in his map delineating the distribution of temperature of the Gulf Stream. An underlying polar current of cold water, even in the more southerly explorations, is likewise clearly established. The position of this mountain chain in affecting the temperature of the air along the coast, as well as the water overlying it, has been as clearly demonstrated.

Lieut. Maury, in his pilot chart of the North Atlantic, has recorded the direction of the wind for each month in the year with great accuracy. The information contained in his chart is taken from the most authoritative sources, and, in some instances, extends back as far as 1810. This chart subdivides the ocean into squares of five degrees each of latitude and longitude, and the monthly observations within each subdivision are made to extend over this surface. For the purpose of the present inquiry, subdivisions of a single degree would have afforded greater definiteness; but the facts, deduced from the chart as it is, are of the highest value. As this inquiry is confined to the summer months, when invalids in search of health, or those who are well in pursuit of a more temperate air than is to be found in the cities, or even in rural districts, visit the sea-shore, the deductions from the chart will be confined to the months of June, July, and August, and to that portion of the Atlantic stretching along the sea-coast.

In the subdivision between lat. 30° and 35°, and lon. 70° and 75°, embracing the sea-coast from St. Augustine, in Florida, to Cape Hatteras, in North Carolina, there were made ninety-nine observations of winds in the month of June. Of these forty-one, or nearly one half, were from the south and south-west. In July, eighty-four winds were noticed, of which fifty-two came from the south and south-west. In August, one hundred and thirty-eight, of which sixty came from the south and south-west.

In the subdivision embracing lat. 35° to 40°, and in lon. 70° to 75°, extending from Cape Hatteras to Cold Spring, New Jersey, three hundred and fifty winds were observed in the month of June, of which one hundred and forty-three were from the south and south-west. In July, three hundred and ten, of which one hundred and sixteen came from the south and south-west. In August, three hundred and sixty-six, of which one hundred and twenty-three were from the south and south-west.

In the subdivision between lat. 40° and 45°, lon. 70° and 75°, embracing Long Island and the southern exposure of the New England coast. In June, two hundred and thirty-one winds were noted, of which one hundred and eight came from the south and south-west. In July, three hundred and eight winds, of which one hundred and sixty were southerly and south-westerly winds. In August, one hundred and eighty-three, of which sixty were from the south and south-west.

These are the facts. The deductions from them are important. It appears from these observations, that of the prevailing winds in the summer months, never less than one third of their number, and, in many instances, one half, come from the south and south-west. It must be remarked, that the winds noted were sailing winds with some degree of force, and not the slight ruffling wind, which, although insufficient for the purpose of rapid sailing, is yet most grateful in its effects over the health and comfort of those so circumstanced as to come within its range. The usual direction of this lighter breeze on the Atlantic in the summer months is from the south and south-west, directly over the current of warm water comprising the Gulf Stream. The winds from this quarter are, for the most part, gentle, balmy, exhilarating, and peculiarly happy in their influence upon the human body. Those from the north and east, on the contrary, are violent, raw, and depressing. While the former should be courted by the invalid, the latter should be as sedulously avoided.

It by no means follows that the wind on shore is the same as that upon the ocean. A very slight obstruction, as an intervening range of hills or indentation of the coast may leave any particular situation in calm, or subject to the influence of a less grateful wind, while the whole surface of the water is swept by a delightful air from the south or south-west. An example of this may be given in the Highlands of Navesink, situated a short distance from New York, and immediately behind the light-house on Sandy Hook point. These highlands, whose sides are covered to the water's edge by a rich growth of vegetation, and are highly picturesque in their effect, are shut off from the south-westerly ocean winds by a small promontory; the effect of which is to render the air upon the sheltered localities calm and oppressive, while on the sandy point, directly in front, and scarcely more than a stone's throw distant, it is agitated by a balmy and refreshing breeze.

The traveller over the New York and New Haven and the New Haven and New London Railways, which pursue an easterly course along Long Island Sound, cannot fail to remark the perceptible difference almost always observed in the temperature, after leaving New Haven for the East. However exalted the temperature may have been, or oppressive the condition of the atmosphere between New York and New Haven, yet he is almost certain to be met by a delightful ocean air from the south-west a few miles east of New Haven, and which accompanies him on his passage to New London. This is due to the configuration of Long Island.

Near New York, the northern shore of Long Island rises into elevations of greater or less extent, but sufficiently so, at most places, to intercept the sweep of wind from the ocean on its southern border. These elevations gradually diminish in an easterly direction, until a point is reached a few miles east of New Haven, where the whole island becomes flat and sandy, and but a few miles in width. This low plateau offers but slight interruption to the progress of the southerly ocean winds, and allows them to play over the surface of the water in the sound itself, and fan the opposite New England coast.

I do not purpose to institute a comparison into the various places of summer resort along the sea-coast, but merely to present the facts on which the advantages or disadvantages of any particular locality may be examined and decided. The greatest advantage, so far as air is concerned, is a free exposure to the south and south-west, and a corresponding protection from the north and north-east. This protection from north-east winds is of the greatest importance in diseases of debility, or where the nervous system is at fault. I have often seen the good effects of sea air of weeks rapidly dispelled by exposure to a harsh north-easter of a day or two's continuance; and I would add, that what is beneficial in a state of disease, is equally so in a state of health.

The islands off the coast of South Carolina, as well as the coast of North Carolina, Virginia, and New Jersey, have a greater or less south and south-western exposure. Within these limits are found Old Point Comfort, Cape May, and Long Branch, which have great celebrity as sea-side places, and attract large numbers of visitors. Each of these places is subject, however, to the depressing effects of north-easterly gales. The more southerly points are less affected from this cause than the more northern.

The whole stretch of Long Island on its southern side, which is at present, with but few exceptions, little better than an inhospitable sand-bar drifted up from the waves of the ocean, — that portion of the Connecticut coast to which we have alluded east of New Haven, as well as Rhode Island and a part of Massachusetts, enjoy in the highest degree the advantage of exposure to the south and south-west, and are, at the same time, best protected from the winds from the north-east. Newport, with many disadvantages, not the least of which is the compactness of its houses, and their almost total exclusion, in the more populous part of the town, from a sea-view, enjoys a world-wide reputation as a seaside residence, for which it is wholly indebted to the salubrity of its air, derived from its sheltered position on the one side, and its free exposure upon the other.

Many sheltered positions may, doubtless, be found on the coast south of Long Island, uniting many, if not all, the advantages already pointed out, which it is earnestly to be hoped will be discovered and improved. The advantage of seaside resorts, in such positions as to render them available for those whose occupations or means do not permit them to take long journeys, cannot be too highly estimated.

2. Fluctuations of Level in the North American Lakes. By Charles Whittlesey, of Cleveland, Ohio.

Since the year 1838, I have observed the changes of level that occur in Lake Erie and Lake Superior, and have collected, so far as I know, all the observations made by others on these and other lakes.

These observations go back to the year 1810, with considerable regularity; and beyond that date to the year 1796, we have some information that may be relied upon. The results are in the course of publication by the Smithsonian Institute, and for this reason, I do not now propose to do more than call the attention of the Association to one phase of the phenomena of fluctuation.

During several years I have been enabled to make, or have been able to procure from others, daily, or in some cases tridaily, readings of the stage of the water. Of this kind, the most valuable and the most complete are those of Col. T. B. Stockton, and Geo. C. Davies, Esq., of Cleveland; John Lothrop, Civil Engineer, Buffalo; and M. P. Hatch, Esq., of Oswego, New York; Gen. Henry Whiting, A. E. Hathan, and Dr. Douglass Houghton, of Detroit; Capt. B. Stanard of Cleveland, and Mr. H. T. Spencer of Rochester, under the direction of Prof. Dewey; and also Messrs. Wm. Finney and M. B. Sherwood, under the direction of John Burt, Esq., at the Sault St. Mary's Canal, have contributed valuable registers. There are, by these numerous readings, *three* kinds of fluctuations found to exist, which are due to distinct causes.

There is, 1st, A general rise and fall extending through *long periods* of time, but without any regularity in the period; which I have styled the *secular fluctuation*.

2d. An annual rise and fall, that occur regularly within the period of *each year*, without reference to the general stage of the water, which is called the *annual fluctuation*.

3d. A local, fitful, irregular oscillation, of a few inches to a few feet, not to be predicted; its period of oscillation being from three to five minutes, and which continues from one to twenty-four hours. I have no difficulty in explaining the general rise and fall in our lakes. It occurs, no doubt, from the differences of the seasons, acting through a long period over a large area. This explanation has not been rendered certain by meteorological observations, because there are too few of them as yet made within the region of the lakes. Starting, however, without any hypothesis, we should conclude that there *should be changes* of level, unless every season was precisely alike from year to year, and from generation to generation.

The lakes are merely the reservoirs of drainage for the country, whose streams discharge therein. The surplus water passes off by the river St. Lawrence as a final outlet. The several lakes are mere ponds of water, caused by natural obstructions along the course of the stream, like the dams constructed by men on smaller streams. Because the extent of country drained is very large, and the climate various, it requires a series of years for the meteorological differences of the seasons to become equalized, and produce general results.

In wet and cold seasons, the river Mississippi is found to be higher than in cold and dry ones. The same is witnessed on the Ohio and all lesser streams; but the shorter their course and the smaller the tract of country which is drained, in just this proportion is the effect more rapid.

In the case of a river like the Ottawa or the Hudson, the rains that fall, or the snows that melt, in the region at their sources, produce high water in a few days, or, perhaps, in a few hours.

On the Mississippi, the Oronoco, and the Ganges, it is months before the inhabitants of the lower portions of these rivers perceive the rise of the water. Considering the St. Lawrence as one great stream, with branches reaching to the Mesabi range, and to the prairies of Wisconsin, the result would be still more tardy, on account of the repeated checks and delays in the flow of its waters through so many pools of still water. The course of the prevailing winds has its effect in increasing or retarding the flow of water, and the amount of vapor taken up in this lengthened passage is very variable.

That there should be found a change in the general level of the *North American lakes* is nothing mysterious, and nothing that may not be readily explained by visible causes, like the fact of change of level in lesser streams and pools of water. The greatest range of the "*secular fluctuations*," between the most extreme high and low stage, determined by the *yearly mean*, is for Lake Erie, 4 feet 5 inches. The greatest extreme in the same lake, determined by the *monthly mean*, is (6) *six feet*. The extreme temporary difference from the highest known stage, June 25, 1838, to the lowest known stage in February, 1819, is about (7) *seven feet*.

There are evidences along the shores of Lakes Huron and Michigan, that the extreme fluctuations there within the life of trees that are now living has been about (12) *twelve feet*. The observations upon the two last-named lakes are few. According to them, the greatest general difference of level is about (6) *six feet* since the year 1819. On Lake Superior, since 1845, when my observations commenced there, the extremes of fluctuation are about (3) *three feet*.

On Lake Ontario, by Mr. Hatch's tables, between 1838 and 1854, the greatest range was (4 feet 9 inches) *four feet nine inches*. As to the second class of variations, those annual changes that occur systematically within the period of each year, — they are not, in general, greater than one foot and one half.

The registers in my possession do not in any degree confirm the traditionary and popular belief of a regular rise and fall in every period of *seven years*.

From 1819 to 1838 there was a continual rise during a period of *nineteen years*.

From 1838 to 1841 a regular decline for three years.

From 1841 to 1851 it has been fluctuating up and down at short intervals. Going further back, it is reported that from 1796 to 1811, the water was low, — fourteen years. From 1811 to 1816 continually rising, — *five years*, — and from 1815 to 1819 always falling, being a period *of three* years. Throughout all these changes there happens to be no period of *seven years;* but I found that nineteen twentieths of the inhabitants of the lake coast firmly believe there is a change from high to low exactly at the expiration of that number of years.

The season or period of highest water within the year is not the same in all the lakes. In Lake Superior it occurs in the month of September or October, and the low-water in February or March. In Lake Erie, June is the high-water month, almost without exception and the months of February and March are the low ones. It is very much the same on Lake Ontario.

With all the lakes, their affluents discharge less water in the winter than in spring. For this reason they are all lower in the winter months than they are in June and July, ranging from *a foot* to a *foot and one half*, and this difference exists without reference to the general stage of the water, whether high or low.

The explanation of this for the lower lakes is, therefore, very simple; it is owing to the temporary surplus of the streams, and the diminution of evaporation. For Lake Superior, its late climate and prevailing winds must be taken into account. Low-water occurs there about the same time as it does in Lake Erie, in February or March; but the maximum height of the annual flood does not take place till September or October, instead of in June or July.

I account for this by the lateness of the thawing in spring, which is from four to six weeks behind Lake Erie; by the greater size of Lake Superior, requiring more time to fill up; by the want of large streams and of other lakes that discharge into it, and also by the prevalence of easterly winds in the spring, that keep back its waters.

The phenomena to which it is my intention to direct the attention of the Association belongs to the *third class* of fluctuations, to the irregular, temporary, and fitful oscillations which occur without any visible cause, but which have attracted the notice of all travellers, from the times of the Jesuit fathers. Although these pulsations were the first to be noticed, and although they excited the astonishment of all beholders, filling the imagination with vague terrors, and the reasoning powers with speculations, this class of movements is the last to receive examination. Dr. Jackson styled them the "Barometric wave" of Lake Superior, from an opinion that they are due to sudden variations of atmospheric pressure. I do not repeat here the various descriptions that have been given by the Jesuit missionaries, and the English and American travellers, since the country around the lake came into the possession of Great Britain and the United States. The accounts are very numerous and graphic, and are probably familiar to those who hear me. In clear, calm weather, when the surface of the water is perfectly placid, and no clouds or winds are visible in the circuit of the horizon, a succession of short swells arise upon the lake, and rapidly approach the shore. Some have represented them as of a terrific height, rolling onward with a bold and foaming crest, threatening the light canoe of the voyager with immediate destruction.

But I have never witnessed any that exceeded two feet (2) in height in stormy weather, and in calm weather not exceeding one and one-half feet ($1\frac{1}{2}$).

I had an excellent opportunity to study this wave, not only during ten years of exploration on the waters and shores of Lake Superior, but also during two years' residence at the mouth of Eagle River, a small creek that discharges into the Lake. The pulsations were here more marked, as they entered the still water of the creek, than they were as they broke upon the shore. Among several hundred observations upon the time of the oscillation, the period of a complete movement from one reflux to another is in average *four and one half minutes* ($4\frac{1}{2}$). For present purposes I do not deem it necessary to transcribe those observations, but give the general results. The vertical range of the wave in the Lake is about *four inches* (4). It always comes in from the open sea in a line *parallel* to the shore. The time between the breaking of each wave upon the beach of course corresponds to the period of the oscillations, as observed on my water-gauge, in the creek, ordinarily 4 to $4\frac{1}{2}$ minutes.

The pulsations are less regular in stormy and windy weather than in still weather; and the vertical range is greater. My memoranda show, however, that they occur in all conditions of the atmosphere,—when it is calm, when the wind is off shore, or along shore, or on shore, and when it is blowing a gentle breeze, as well as in a gale amounting to a resistless tempest.

The crest line of the wave is always *parallel* to the coast line, without regard to the direction of the wind. In still weather, or when the Lake is but slightly agitated, as by a breeze off land, it is a beautiful sight to witness, over a long stretch of coast, these low, regular undulations, as they break languidly upon a sandy beach. This continues an hour, two hours, ten hours, a day, and even longer; but are seldom witnessed beyond eight or ten hours at a time.

They occur at night, as well as by day. I have endeavored to discover some connection between the period of their occurrence and the presence of the moon, but as yet without effect.

They are not confined to the upper Lakes, but are noticed equally on Lakes Erie and Ontario, and the smaller lakes of interior New York; and it is very probable that observations made at the sea-coast would detect them there. As yet it is not settled whether they occur in winter, because on the Northern lakes the shores, for several months, are skirted by ice. So far as I am informed, the idea that these undulations are due to barometrical variations in the atmosphere is purely theoretical, and not supported by any observed facts. The first recorded observations of them made in connection with barometrical readings that I know of, are those of Professor Mather, at Copper Harbor, in July, 1847. During the greater part of a day he observed the state of the water, as to flux and reflux. While the oscillations were marked and rapid, the mercurial column was continually moving in one direction: it was *falling* during all the time of his observations.

In truth, it would seem that a movement which is completed in the space of three to five minutes is too rapid to allow itself to be felt in a column of mercury, even if the oscillation is due to variation of pressure. Until near the close of my observations at Eagle River I had no barometer; but in October, 1856, was enabled to compare this movement of the water with the movement of the mercury during *three days* (3). All of this time the barometer was quite regular, and during most of it the mercury *fell gradually*. There were no sudden changes, and the readings of the instrument are very uniform for the length of time it was observed. The instrument was an Aneroid compared with a cistern barometer, and was read at the moment of the culmination of each wave, as well as at the period of its complete reflux or lowest stage. Without entertaining any fixed theory of these phenomena, I have been for several years inclined to look to electro-magnetic causes for an explanation.

I have noticed the Northern Lights in connection with the oscillations, but cannot discover any relation in the time of their occurrence. A series of observations with electrical and magnetic instruments would be necessary to discuss this theory, and as yet none have been taken. These observations should be made at the same time at different and distant points on the shore of one of the lakes. As this class of movements is more frequent and more prominent on the waters of Lake Superior than on the lower lakes, it would be preferable to take them there.

If there is a barometric wave in the atmosphere corresponding to that in the water, it should, as it seems to me, be due to agencies that are seated on the land. In cloudy, and especially in foggy, weather, the movements are most striking. The mountain ranges that approach the coast no doubt affect the electrical condition of the atmosphere. They condense its vapor, especially when winds sweep over their summits. The increase or diminution of vapor changes the quantity and the force of the electrical fluid. The presence or absence of the sun, which depends upon the degree of cloudiness, produces the same effect. We observe every day that storms arise in this way, particularly in the afternoon of hot days; the atmosphere is agitated, lightning is produced, and rains fall. I think it not unphilosophical, therefore, to look in that direction for a solution of this question. To surmise that a vibratory motion is thus given to the water, or, more probably, to the atmosphere that rests upon the water and communicates with it, which movement is always to and from the general trend of the coast. This, over local and limited spaces, would not be appreciable to the barometer, being rather an agitation within the atmospheric column than a change in its total weight.

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3. Notes on the Measurement of the Base for the Primary Triangulation of the Eastern Section of the Coast of the United States, on Epping Plains, Maine. By A. D. Bache, Superintendent U. S. Coast Survey.

[Communicated, by authority of the Treasury Department, to the American Association for the Advancement of Science.]

The reconnaissance for a base of verification at the eastern extremity of the primary triangulation in section I. of the coast was commenced by Charles O. Boutelle, Esq., and Major Henry Prince, U. S. A., assistants in the Coast Survey in 1853, and continued through 1854 and 1855. The absence of long and straight sea beaches on this coast rendered it absolutely necessary to look for an interior site.

The reconnaissance resulted in the selection of Epping Plains, Washington county, Maine, as the most suitable site for the purpose, considering the character of the ground itself, and the facility of connecting the ends of the base with the primary triangulation. In this selection and the examination of the plains, these officers were much assisted by the local knowledge and the kind offices of J. A. Milliken, Esq., now of Cherryfield, to whom I beg leave here to return the thanks of the Coast Survey.

Major Prince being relieved from the survey, the final minute examination of the site, and the determination of the best line which could be obtained on the plain, devolved upon Assistant Boutelle, who was assisted at different times by Sub-assistant J. A. Sullivan, Lieutenant J. C. Clark, U. S. A., and Mr. F. P. Webber.

Epping Plains or "Barrens," as they are called, lie between the Narraguagus and Pleasant Rivers. They present a moderately rolling surface of sand, generally destitute of trees, except in the lower and swampy parts, and are traversed by sand ridges of different elevations, resembling very much the surface which the sounding line develops in such regions as the Nantucket shoals, at present below the surface of the water.

The plain is quite elevated, and falls suddenly from an irregularly curved margin by a steep slope to a lower plain or a wide valley.

Portions of the plain are strewed with boulders of various sizes, some of them containing not less than four thousand cubic feet, and of various granitic materials. Schoodiac hill was found to limit the position of the base, so that the problem became to draw the longest line through a point at the base of that hill, the ends of which would be easily visible from the secondary and primary stations.

Before the final selection of the line, a topographical survey was made, under the direction of Assistant C. O. Boutelle, by Sub-assistant J. A. Sullivan and Mr. Webber, and the profile was studied upon a sketch of the plain made by Lieutenant Clark.

In 1856, I examined the site, and took steps to obtain the necessary estimates of the cost of preparing it for measurement. The profile of the line as graded gives a good general idea of the ground, as it varied but little from the natural profile (see Sketch). The whole length of the line is about 8,716 metres, or 5.4 miles. Its general direction is E. 16° S. (*true bearing*). From the eastern end, for about four miles, the plain is quite level, rising in the first mile pretty regularly about fifteen feet, descending nearly as much in the next to rise by the same quantity in the third mile. It then runs along an elevated level for a fourth of a mile, and descends gradually to the rougher part of the base, which is included between the three and three fourths miles from the east end and the western end of the base.

This line was skilfully graded by Mr. Boutelle so as to follow the natural surface when the grade did not run above three degrees, and to give as long slopes as possible of the same grade for the convenience of measuring (see Sketch).

As it was found more economical to make the temporary embankments than to excavate, a profile giving a considerable excess of embankment was selected.

This was executed in the cheapest way which would give stability for the time during which it was required to stand. The least width was twelve feet, of which nine feet was on the south and three feet on the north side of the line to be measured. The base was very carefully aligned. High signals were placed over the termini, which are intervisible. On the Schoodiac, a signal of moderate elevation is visible from both, and the distances between this point and the termini were gradually subdivided until the smallest limit, the distance easily reached by a small transit, was obtained.

The verification of the alignment at different points of the measurement, when the seeing was good, was complete.

In all these preliminary operations Mr. Boutelle was assisted by Sub-assistant J. A. Sullivan and Mr. Webber.

His grading party consisted of the farmers and lumbermen of the district, who served with great cheerfulness and skill in the use of the heavy implements for rough grading.

One of the greatest difficulties was the removal of such boulders as were in the line, many of them being of such size as to require blasting to break them up, and some being actually removed to the required distance from the line by heavy blasts.

The signals erected at the two ends are very substantial, each forty-three feet in height to the top of the tripod, and fifty-three to the cone which surmounts them.

The base apparatus, already described before the Association, and described and figured in my report for 1854 by Lieutenant E. B. Hunt, of the Corps of Engineers, was used in this measurement, preliminary trials being made in the office to test its steadiness under the greatest inclinations to which it would be subjected, and the accuracy of the surface upon which the measuring stem traverses, and which determines the length of the apparatus. I was assisted in the measurements by Assistant G. W. Dean, Professor Fairman Rogers, who volunteered for the purpose, Sub-assistants Goodfellow, Stephen Harris, and Sullivan, and Mr. Thomas McDonnell, among whom the different operations were divided.

The usual comparisons of the apparatus with the standard six-metre bar were made before and after the measurement, to ascertain that no change had taken place in the length from damage by transportation, and to add to the results of former comparisons.

The measurement was begun at the west end of the line on Saturday, the 18th of July, but the next week proved so rainy that it was only resumed in earnest on Monday, the 27th.

The work of the first Saturday (24 tubes) was remeasured on the following Monday with precisely the same result as to length, the end of the second measurement falling exactly on the marks which had been placed as terminating the first, and which were fine dots upon the head of a copper nail placed in a stake, some eighteen inches in length, driven into the ground until its head just projected above the surface. The position of the mark was determined and verified, as all others of the sort in our measurements, by using a transit placed at right angles to the line and at a moderate distance from it.

This was on a descending slope of the strongest grade adopted, and there was a difference of temperature of some five degrees in the two measurements. On Tuesday, a length of eighteen tubes, which had been measured on Monday, was remeasured with an identical result. This was on an ascending slope.

On Monday the work was in part interrupted by the arrangements for photographing the apparatus, on Tuesday by a fog, and on Wednesday by showers in the beginning of the day; we made, however, half a mile on both days. On Wednesday began a series of four unbroken days, during the first of which we measured about seven eighths of a mile, and on the three others, a mile, or more than a mile each day, reaching the east end of the base on Monday evening. Thus, counting in the broken days, 5.4 miles were measured in eight days.

This time included the marking of five permanent points near to the ends of the successive miles, where stone posts have since been placed. The ends of the base will be marked by regular monuments. The base of the monument at the west end is cut from the ledge of rocks upon which the signal stands.

By the kindness of Professor Fairman Rogers, I have been enabled to collect approximately some of the statistics of the measurement in a tabular form (No. 1). A second table contains the comparison with the other five Coast Survey Bases which I have measured.

EPPING BASE. — TABLE I.

Whole length of Base in tubes	1453
" " " metres	8714.52
1.4250 added at East Base, making	8715.97
or 28,594 feet, or about 5.4 miles.	
Difference of level between highest and lowest points	104 ft. nearly
Mean level of base above mean tide	251 ft. or 76.5
Approx. corr. for reduction to the level of the sea	$0^m.10438$
or 4 inches, nearly.	
No. of tubes inclined	643
" " level	810
Ratio of tubes inclined to the whole number	0.442, nearly
" " level " " "	0.550
Correction for versed sine for whole base	$2^m.804$
or 9.2 feet to be substracted.	

			Ratio to whole number inclined
Maximum inclination	3° 14′		
Number of tubes inclined	3°, and over	31	0.048
" " "	2° 30′ "	234	0.364
" " "	2° "	79	0.123
" " "	1° 30′ "	120	0.186
" " "	1° "	110	0.171
" " "	0° 30′ "	21	0.032
" " "		48	0.74
		643	

Greatest day's work, 281 tubes, 1.05 miles in $11^h\ 10^m$ working time.
Averaging 1 tube in $2^m\ 27^s$.
Greatest No. in 1 hour, 37, or $1^m\ 37^s$ for each tube.

TABLE II. — COMPARATIVE TABLE OF THE MEASUREMENTS OF SIX UNITED STATES COAST SURVEY BASES.

	Dauphine Island.	Bodies Island.	Edisto Island.	Key Biscayne.	Cape Sable.	Epping Plains.
Whole No. of Tubes measured	1777	1807	1787	965	1072	1453
" " days employed	17	10	13	9	8	8
" " hours "	143h 17m	81h 08m	97h 28m	66h 31m	46h 26m	69h 43m
" " tubes level	961	1496	862	473	994	810
" " " inclined	816	311	925	492	78	643
Average length of working day	8h 25m 7s	8h .07	7h 30m	7h 23m	5h 48m 18s	10h 07m
" time of one tube	5m 32s	2m 54s	3m 22s	4m 20s	2m 51s	2m 58s
" No. of tubes per day	104.5	180.7	137.5	107.2	134	181.6
" " " " of 9 hours	108.0	197.9	165.9	130.0	200.0	187.2
" " " per hour	11.85	21.98	18.40	14.40	22.47	20.8
" plus inclination	17′.6	16′.2	24′.5	31′.0	12′.0	1° 53′
" minus "	16′.6	19′.6	23′.0	26′.0	10′.0	1° 54′
" of greatest plus inclination	40′.8	23′.7	55′.4	58′.0	14′.0	2° 52′
" " minus "	42′.6	29′.5	48′.4	54′.0	11′.0	2° 46′
" temperature Fahr.	84° 50	52° 01	59° 50	82° 90	87° 90	70° 0

The photographs of the apparatus and operations which I submit to the Association were taken by Mr. Black, of the firm of Whipple & Black of Boston, who exerted himself especially in the matter, and succeeded, under many disadvantages from variable weather and the roughness of field arrangements for photography, in making satisfactory representations.

The views of the apparatus and operations, include the placing the apparatus over a mark, the aligning, the setting of the trestles in advance of the measurement, the transfer of the measuring tube, the making of contact. The comparing apparatus and tent are also shown.

The sketch shows the topographical features of the ground, and gives the profile of the base as graded for measurement.

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4. Notice of the Determination of the Longitude of Fernandina, Amelia Island, Florida, by means of Chronometer Exchanges from Savannah, Georgia. By A. D. Bache, Superintendent, and Chas. A. Schott, Assistant, U. S. Coast Survey.

[Communicated by authority of the Treasury Department.]

It is proposed to connect the triangulations of the Atlantic coast and of the Gulf of Mexico by a series of triangles across the peninsula from near Fernandina to near Cedar Keys, the termini of the Air Line Railroad. The importance of this connection will be obvious when the distance around the peninsula, and the nature of the triangulation which will envelop it, are considered. A reconnaissance had shown such a triangulation to be practicable, and it was desirable to keep the general direction of the line already referred to. The latitude of the two termini had been observed, and the longitude of a point on one of the Cedar Keys.

The determination of the longitude of Fernandina would furnish the remaining element needed, and also an important datum for the geodetic work of the coast of Georgia and Florida, already in progress. The longitude of Savannah had been determined by telegraph, and the connection of Fernandina with it was easily made by the transportation of chronometers in the steamers running twice a week between Savannah and the St. John's, and stopping at Fernandina.

It is proposed to give an account of the operations of this expedition, with the results for personal equation, for the performance of the chronometers under different circumstances, and others, and of the final determination of the difference of longitude.

The incidental results will probably have more interest for the American Association than the mere longitude question, though as the two best maps differ some nine miles in the longitude of Fernandina, the final result must, in a practical point of view, be esteemed of great importance.

Early in the spring of the present year, arrangements were made for executing this work by a party under the immediate direction of the Superintendent of the Coast Survey. The old astronomical station near the Exchange, in the city of Savannah, was rearranged and furnished with suitable instruments. This station is on the bank of the Savannah River, and forty-four feet above mean low water, commanding a good view of the heavens both to the south and the north. It has one drawback, namely, that the stone supporting the transit instrument rests upon an arch, and that the vibration from heavy vehicles passing in the street is communicated to the instrument, affecting in a degree the accordance of individual results, but not sensibly the final determination of time.

The outfit of the station consisted of transit instrument No. 3, by Troughton and Simms, with a focal length of forty inches and a magnifying power of seventy, of an observing chronometer (sidereal), No. 1707, by John Fletcher, and of the usual meteorological instruments. The observations generally were made at this station by the Superintendent, replaced for a few days, in the early part of the work, by Assistants Charles A. Schott and Charles O. Boutelle. Mr. J. H. Toomer was the aid in the party. The transit was mounted on the 25th of March, and the observations were closed on the 30th of April.

At the other end of the line, a smaller transit, by Würdemann, C. S. No. 10, was mounted at the station near the railroad wharf, occupied the year previous by Sub-assistant Edward Goodfellow for the determination of the latitude of Fernandina. The observations were made by Assistant Charles A. Schott and Mr. J. E. Blankenship. The transit instrument used has a focal length of twenty-six inches, and a magnifying power of nearly thirty-five. The observing chronometer was by J. Hutton, No. 311 (sidereal). The same meteorological instruments were observed as at Savannah, daily at 6 A. M. and at 2 and 10 P. M. The barometers and thermometers used were afterwards compared for index error. This station is but a few fect above the level of the tide of Amelia River, and the wooden blocks upon which the transit was placed actually reached that level, which caused an instability in azimuth, requiring a determination of that element by suitable stars every observing night. The observations here were begun on the 1st of April, and continued until the 27th of the same month.

Observations for personal equation in observing transits were made at Savannah at the beginning of the work and near its close, and at Fernandina at the close. No difference appeared in the values found by the two series at Savannah, nor by the use of the two different transit instruments.

Ten chronometers were employed, divided into two sets, and packed in two of the boxes used in the chronometer expedition of 1855, the sides and top and bottom of which were thickly padded to prevent abrupt changes of temperature and the effects of jarring. Eight of these chronometers were regulated to solar time, the other two as comparing chronometers, so as to lose about four minutes in mean time in twenty-four hours. With set No. 1 was the thermometric chronometer of Mr. Bond, without compensation, and indicating the mean temperature of its exposure by its gain or loss. Each boat of the line between Savannah and Fernandina made usually one trip to and fro within a week, the days of departure and arrival being so arranged as to require that the same set of chronometers should go and return in the same boat, the first boat from Savannah in the week not returning in time to permit the rating of the chronometers for the trip of the second. Mr. D. Hinkle, Aid, was charged with the exchange of the boxes, and accompanied one of the sets going and returning. The other set was placed in charge of the captain of the boat, and both were always kept in the captain's office. Twice the observers, when changing pieces, accompanied the instruments. They were transported by hand between the wharf and the stations. This precaution secured general uniformity of rate. Some of the chronometers were supplied with a thermom-

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eter, which was read at each comparison. The average duration of the trip was twenty-nine hours from comparison to comparison, these being made as near as possible to the time of arrival and departure.

Diagram No. 1 shows at a glance the number of trips made, and the dates of departure and arrival of each set of chronometers at the two stations. One of the chronometers of the set No. 2 being required for other use was withdrawn, and another instrument, kindly loaned by Mr. Richmond, of Wilmot & Co., of Savannah, substituted for it about the middle of the operations, causing no inconvenience from the change.

The weather was generally favorable, the clear sky permitting regular observations, and only one very abrupt change of temperature occurring during the series. As far as practicable the same stars were used at both stations. The routine of observations was as follows: 1st. The level was read; 2d. A low star was observed for azimuth; 3d. Two zenith stars for time; 4th. The level was again read. Next the instrument was reversed, the level read, two stars near the zenith, and one low star observed, and the level again read. Occasionally special observations for collimation were made by reversals on Polaris on the middle wire. The Savannah transit had seven wires, and the Fernandina nine. The probable error of time for the transit over all the wires of each transit was about the same; namely, $\pm$ $0^s.11$. This includes any error of right ascension. The places of the stars were taken from the Greenwich Nautical Almanac. The equatorial intervals of the wires of the two transits were known to within a probable error of $\pm$ $0^s.05$ and $\pm$ $0^s.04$ respectively.

The value of one division of the level of transit No. 3 was found to be $1''.05 \pm 0''.02$, and of No. 10, $0''.73 \pm 0''.02$. Observations made for the purpose show that the pivots of the first-named instrument are sensibly equal, and gave for the second a correction which, at the greatest, is $0^s.02$, the clamp pivot being the larger.

Fifteen hundred and twelve transits were observed at Savannah, and twelve hundred and sixty-nine at Fernandina. They were all reduced by the method of least squares.

In observing, time was marked for the superintendent by Mr. Toomer. Mr. Schott marked time for himself. The records were kept by Mr. Toomer and Mr. Blankenship.

Personal Equation. — Observations on alternate wires for personal equation between the superintendent and assistant Boutelle showed no sensible difference, time being marked for both by Mr. Toomer. Ten stars, observed on April 4th, gave for personal equation $-0^s.01$, $\pm 0^s.02$; and seven stars on the 6th, $+0^s.05$, $\pm 0^s.02$. The following are the results for personal equation between the superintendent and assistant Schott:—

Locality.	Date.	Transit No.	Magnifying Power.	No. of Stars.	B S.	Prob. Error.
	1857.					
Savannah.	March 30.	3	70	23	$-0^s.62$	$\pm 0^s.03$
"	April 22.	3	70	10	-0.64	0.04
"	" 23.	3	70	12	-0.50	0.03
Fernandina.	" 26.	10	35	12	-0.73	0.10

The result is $B-S = -0^s.58, \pm 0^s.04$, which has been applied when necessary. The separate results are remarkably accordant when the variety of circumstances is considered. The first were obtained when the first observer had the day before arrived from a fatiguing journey; and the second had had several days of practice. In the second and third both observers were in full practice, the second having had but a brief interval from work for the short trip from Fernandina, by water. The practice had been, it is true, with the smaller instrument at Fernandina. In the last series, both were fresh from the short trip to Fernandina; the first had been in full practice with the large instrument, but had not used the smaller one. The probable error of one determination varies under the different circumstances, within the limits of $\pm 0^s.10$ and $\pm 0^s.14$, under the circumstances already stated, while the average is still preserved. In a few cases, the first observer took the stars' passage over the first four; and the last, the second three wires of the transit.

The results for personal equation were grouped for stars observed; for illumination east, and then west; for the two observers leading respectively; for stars north and south of the zenith; and for low and high stars, without sensible deviations from the mean value.

To determine how much of this large personal equation was due to the difference between the marking of time by Mr. Schott and Mr. Toomer, and how much to the difference of observation of the stars' passage over the wires, Mr. Toomer was directed, on the 26th of April, to mark time for both observers. Eleven stars were thus observed, with a personal equation of B–S = $-0^s.30$, $\pm$ $0^s.03$; thus, $0^s.28$, or nearly one half of the personal equation was due to the difference in marking time or ear perception; and the other half to the difference in the observation of the transit or the eye perception. This supposes the tapping to be simultaneous with the perception of the transit; and, on the average, this is probably really so.

Diagram No. 2 shows the separate values found for personal equation, and presents some results worthy, when time and opportunity serve, to be further pursued. The steadiness to the mean, with large accidental fluctuations on the first night, and the running up on the nights of the 23d and 24th, towards the close, and down on the 26th, are very curious.

Temperature Compensation.—The compensation of chronometers is never perfect for any considerable range of temperature. Prof. W. C. Bond pointed out, in the Coast Survey Chronometer Expedition between Cambridge and Liverpool, the consequences of this, and elaborately examined the rates at the different temperatures to which the chronometers were likely to be subjected in the different voyages. Diagrams Nos. 3 and 4 show conclusively the over-compensation of the observing chronometers used at the two stations, the curves of rate and of temperature corresponding remarkably.

Twenty-four conditional equations between the observed rate and temperature were found for Savannah, and seventeen for Fernandina; and the solution of the normal equations gave the correction to be applied to the rate of each chronometer for temperature. The results were thus reduced to a mean temperature, and the remaining irregularities were equalized graphically. These corrections were applied during the interval between the time of observations of transits and of the comparison of the chronometers, in determining the error of the chronometer or sidereal time for the epoch of the comparison.

Chronometer Comparisons.—Each set of chronometer comparisons consists of a double operation, the repetition being considered necessary to insure correctness. The chronometers beating half seconds, there is a coincidence of beat between the comparing chronometer and a mean time chronometer every minute, and between the comparing and sidereal chronometer every minute and a half. The comparing chronometer was observed in turn in a regular order with each of the others, passing from one end of the box containing them to the other, and then back again to the first. To the mean of the observed times of coincidence of beats, a small correction is applied for the gain of the comparing chronometer on mean or on sidereal time, giving the chronometer time at the common epoch of comparison.

To these times the corrections already referred to were applied.

Stationary and Travelling Rates. — The next step is the determination of the stationary and travelling rates of the chronometers. The stationary rates resulted directly from the observed change (corrected), and the interval between the time of arrival and departure at each station. In making these reductions it was found most convenient to regard the chronometers as sidereal ones, with a large rate. Stationary rates were reduced for eight intervals, four at each station, for each chronometer.

The travelling rates were obtained by deducting from the whole interval between the departure from, and return to, the same station, the shorter stationary interval at the other station, to obtain the sidereal travelling interval, and from the whole change of chronometer time, the change during the stationary interval, to obtain the travelling change.

These travelling rates were ascertained for the trips from Savannah and back again, as well as from Fernandina and back, and by using these, approximate values for the difference of longitude were obtained.

The error arising from the assumption of equal travelling rates for the outward and home trip of each journey, implied in this process for deducing the travelling rates, is partially corrected by taking the mean of the longitudes from and to Savannah, and from and to Fernandina.

Taking the mean of seven values for travelling rates of each chronometer, the stationary and travelling rates compare as follows: —

Set No. 1.			Set No. 2.		
Chron's.	Mean Stationary Rate.	Mean Travelling Rate.	Chron's.	Mean Stationary Rate.	Mean Travelling Rate.
	s.	s.		s.	s.
1,507	— 484.75	— 484.12	1,508	— 485.42	— 485.60
175	+ 232.07	+ 232.75	184	+ 230.82	+ 229.92
177	+ 236.48	+ 237.09	193	+ 236.40	+ 235.53
191	+ 236.55	+ 236.60	{ 195	+ 235.86	+ 236.84
301	+ 231.07	+ 231.19	{ 442	+ 240.44	+ 239.67
			1,285	+ 235.06	+ 234.78

This table shows that travelling affected the two sets differently, all the five chronometers of the first set, and only two of the second set, lost by travelling. The separate comparisons of the travelling rates, in the different trips, agreed well among themselves.

Having the travelling rates deduced from the voyages or trips from and back, the travelling rate for each trip was deduced by the application of the method of least squares. The following table gives a specimen of the result for one of the chronometers of the second set (No. 1,285).

Travelling Rate of Chronometer.				Duration of Trip.	
From Voyage to and fro.		From Single Trip.			
From Sav. and back.	From Fern. and back.	From Sav. to Fern.	From Fern. to Sav.	Savannah to Fern.	Fernandina to Sav.
s.	s.	s.	s.	d.	d.
235.31		235.25	235.38	1.092	1.073
	235.50	235.64		1.076	
235.02			234.50		1.202
	234.61	234.72		1.322	
234.60			234.49		1.361
	234.17	233.81		1.265	
234.24			234.76		0.995

Comparing the travelling rates of the separate trips with the average travelling rate before found for the voyage or trip to and fro, the differences will be seen to be quite small, showing the satisfactory character of the first hypothesis from which the approximate longitude was deduced.

Having thus the individual travelling rates for the several chronometers of each set, a difference of longitude was deduced by using it in combination with the stationary rate.

The following table gives the separate longitudes deduced from each chronometer.

Set No. 1.			Set No. 2.		
Chron.	Diff. Long.	Prob. Error of each Chron.	Chron.	Diff. Long.	Prob. Error of each Chron.
	m. s.	s.		s.	s.
1,507	+1 29.47	±0.11	1,508	+29.78	±0.09
175	29.19	0.30	184	30.40	0.15
177	29.59	0.10	193	29.45	0.20
191	29.57	0.07	195	30.29	0.29
301	29.79	0.05	442	29.93	0.10
			1,285	29.94	0.09

Mean $+1^m\ 29^s.73 \pm 0^s.07$.

This table shows such a decided difference between the longitudes given by the two sets of chronometers, or what may be called a personal equation between the two sets even after the application of corrections for the temperature deduced for each instrument, that it was deemed advisable to try whether if the two sets were exposed to identically the same circumstances, this difference would remain. They were, therefore, carried side by side from Savannah to Fernandina and back, and carefully rated at both places. The resulting longitude

for set No. 1 was $1^m\ 28^s.6$,

and for set No. 2 was 1 29.8,

agreeing with the former results.

If, in accordance with the method of reduction of these observations we allow weights according to the inverse squares of the errors, allowing also for the difference in duration of the trips, the several chronometers would show very different values for use on such an expedition.

Set No. 1.			Set No. 2.		
Chron's.	Prob. Error.	Weights.	Chron's.	Prob. Error.	Weights.
1,507	±0.11 s	76	1,508	±0.09 s.	114
175	0.30	11	184	0.15	45
177	0.10	106	193	0.20	25
191	0.07	190	195	0.29	12
301	0.05	359	442	0.10	94
			1,285	0.09	129

$\Sigma = 1161.$

Using these weights, we deduce for the final difference of longitude of Savannah and Fernandina

$$1^{m.}\ 29^{s.}.76,$$

with a probable error of $\pm\ 0^{s.}.06$.

The difference between this result and the contradictory ones of two of the best charts of Fernandina, has already been stated in our prefatory remarks.

5. On the Heights of the Tides of the Atlantic Coast of the United States, from Observations in the Coast Survey. By A. D. Bache, Superintendent.

[Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.]

It is well known, that when a bay or indentation of the coast presents its opening favorably to the tide wave, and decreases in width from the entrance towards its head, the tides rise higher and higher from the mouth upwards. The Rev. Mr. Whewell has stated that in a general way the same fact is deduced from the observations on the coast of Great Britain and Ireland discussed by him.

The Coast Survey observations of the tides of the Atlantic Coast, the results of which, from time to time, I have brought before the Association, furnish the means of a complete discussion of heights as well as of times, and very simple generalizations result from their examination. Through the kindness of Capt. Shortland, R. N., and of

Admiral Bayfield, R. N., I have been enabled to extend these results to the coast of New Brunswick, Nova Scotia, and to part of Newfoundland.

I beg leave to make my best acknowledgments to these distinguished hydrographers for the prompt and liberal communication of the results of their observations.

The Coast Survey Observations have been worked up in the tidal division under the direction of Assistant L. F. Pourtales, and I am indebted to him for giving the results the shape desired, and for the diagrams representing them.

The following table of stations on or near the exterior coast line of the United States is taken from the more extended tables of the Coast Survey, omitting stations which are up rivers or bays, except in special cases, the object of inserting which will be obvious.

Table A contains a number for reference, the locality of the tidal station, the State to which it belongs, the latitude, longitude, and the mean height of the tide in feet and tenths.

TABLE A. — HEIGHTS OF TIDES ON THE ATLANTIC COAST OF THE UNITED STATES.

No.	Locality.	State.	Latitude.		Longitude.		Heights in feet.
			°	′	°	′	
1	Portland,	Me.	43	39	70	14	8.8
2	Portsmouth,	N. H.	43	04	70	42	8.6
3	Newburyport,	Mass.	42	48	70	52	7.8
4	Gloucester,	"	42	37	70	40	8.9
5	Salem,	"	42	31	70	54	9.2
6	Boston,	"	42	22	71	03	10.0
7	Plymouth,	"	41	57	70	40	10.1
8	Provincetown,	"	42	03	70	11	9.2*
9	George's Shoals,	"	41	40	67	45	7.0†
10	Monomoy,	"	41	33	69	59	3.8
11	Siasconsett,	"	41	15	70	00	2.2
12	Weweeder,	"	41	15	70	05	1.2
13	Smith's Point,	"	41	17	70	16	2.1
14	Wasque,	"	41	21	70	30	1.7
15	Menemsha,	"	41	20	70	45	2.7
16	Point Judith,	R. I.	41	22	71	29	3.1
17	Newport,	"	41	29	71	20	3.9
18	Block Island,	"	41	10	71	34	2.8
19	Montauk Point,	N. Y.	41	04	71	51	1.9
20	Stonington,	Conn.	41	20	71	54	2.3
21	New Haven,	"	41	18	72	54	5.8
22	Fire Island,	N. Y.	40	38	73	13	2.1
23	Sand's Point,	"	40	52	73	43	7.7
24	Sandy Hook,	"	40	28	74	00	4.8
25	Cold Spring Inlet,	N. J.	38	57	74	45	4.4
26	Cape May,	"	38	56	74	57	4.8
27	Old Point Comfort,	Va.	37	00	76	18	2.5
28	Hatteras Inlet,	N. C.	35	12	75	43	2.0
29	Beaufort,	"	34	42	76	40	2.8
30	Cape Fear,	"	33	52	78	00	4.4
31	Winyah Bay,	S. C.	33	14	79	8	3.8
32	Charleston,	"	32	46	79	54	5.3
33	North Edisto River,	"	32	33	80	13	5.8
34	Port Royal,	"	32	17	80	40	7.0
35	Savannah Entrance,	Geo.	32	02	80	53	7.0
36	Sapelo,	"	31	21	81	24	6.6
37	St. Simons,	"	31	8	81	36	6.8
38	St. Mary's River,	"	30	42	81	36	5.9
39	St. John's River,	Fla.	30	20	81	33	4.6
40	St. Augustine,	"	29	52	81	25	4.2
41	Indian River Inlet,	"	27	28	80	19	2.5
42	Cape Florida,	"	25	40	80	09	1.5

* Major Graham, U. S. A. † Capt. Wilkes, U. S. N.

The following table of tides of localities on the coast of Cape Breton, Nova Scotia, and New Brunswick, is from the observations of

Admiral Bayfield and Captain Shortland. The authorities are given in the column of remarks, which also contains the remarks of Admiral Bayfield on the tidal results communicated by him. I have taken from his table the heights which were derived from the greatest number of observations. The column of means is the average of the heights of spring and neap tides in feet and tenths. The localities are arranged from the north, southward on the outer coast, and in the Bay of Fundy from the entrance up the bay.

From the table of Captain Shortland I have selected only a few localities as specimens, having no wish to anticipate through his generosity the use which he will doubtless make of his own results.

TABLE B.—HEIGHTS OF TIDES ON THE COAST OF CAPE BRETON, NOVA SCOTIA, AND NEW BRUNSWICK.

No.	Localities.	Remarks on Localities.	Latitude. °	Latitude. ′	Long. °	Long. ′	Rise of Tide. Ord. Spring. ft.	Ord. Spring. in.	Ord. Neap. ft.	Ord. Neap. in.	Mean.	Remarks and Authorities.
	Island of Cape Breton.											Admiral Bayfield.
1	St. Ann's Harbor.	Entrance.	46	17	60	33	5	0	3	3	4.1	A complete semi-lunation observed.
2	Sydney Harbor.	S. E. Bar.	46	12	60	13	3	9	2	4	3.1	At full moon, and a day or two before and after.
3	Menadon Harbor.	Near Scataria Island.	46	00	59	50	5	6	3	4	4.4	Good. A complete semi-lunation observed. [bef. & aft. each.
4	St. Peter's Island.		45	36	60	49	6	0	4	0	5.0	At new moon, and a day or two bef. and after. [with sev. days
5	St. Peter's Bay.	Haulover, at head of Bay.	45	39	60	52	5	9	4	1	4.9	Good obs. 4 times observed, twice at full & twice at new moon,
6	Grandigue.	In Lennox Passage.	45	36	61	01	6	4	4	6	5.4	Good. A complete semi-lunation observed.
7	Arichat Harbor.	Jerseyman Island, N. Point.	45	30	61	03	5	0	4	0	4.5	Ditto. Ditto. Ditto. Ext. tides rise 6 ft.
	Nova Scotia.											
8	Canso Harbor.	E, end of Cutler's Island.	45	21	60	59	6	6	4	6	5.5	A complete semi-lunation observed, but tides very irregular.
9	White Haven.	Marshall Cove.	45	15	61	11	6	1	4	1	5.1	A complete semi-lunation. Good observations.
10	Harbor Island.	N. E. Point.	45	08	61	36	6	6	4	6	5.5	A complete semi-lunation observed. Extraord. tides rise 7 ft.
11	Liscomb Harbor.	Pye's Wharf.	45	00	62	01	6	0	4	0	5.0	3 times obs.: at full and new moon, and sev. days bef. & aft.
12	Sheet Harbor.	Watering Cove.	44	54	62	30	6	8	4	6	5.6	Good. Two complete semi-lunations observed.
13	Pope Harbor.	Harbor Island, N. E. Point.	44	48	62	39	6	6	4	2	5.3	3 times obs.: at full and new moon. [Extr. neaps, only 4 ft.
14	Ship Harbor.	Salmon Point.	44	47	62	49	6	5	4	10	5.6	Good. A compl. semi-lunation. Extr. spr. tides rise 7 ft., and
15	Iedore Harbor.	Marsh Point.	44	43	63	00	6	6	4	8	5.6	Two good and complete semi-lunations observed.
16	Halifax Harbor.	Naval Yard.	44	40	63	35	6	0	4	6	5.2	Mean of a complete year's observations with a tide-gauge.
	Bay of Fundy.											Captain Shortland, R. N.
17	Cape Sable.	Cape Sable Isl., Clarke's Har.	43	25	65	39	11	6	4	11	8.2	
18	Ellenwood's Island.	Bird Rock.	43	39	66	04	12	7	7	0	9.7	
19	Yarmouth Harbor.	Fourchue Isl. Light-House.	43	47	66	10	10	9	16	0	13.3	
20	Bryer's Island.	Peter's Island Light-House.	44	15	66	21	20	6	9	3	14.8	
21	Campbell's Island.	Owen's House.	44	54	66	58	25	0	11	0	18.0	
22	St. John, N. B.	Battery Point Rock.	45	16	66	04	26	8	12	0	19.3	
23	Shadwood Point.	Cumberland Basin.	45	54	64	22	50	0	22	0	36.0	

These numbers may be extended beyond the turn of Cape Race, where the coast trends to the west of north, by further results of Admiral Bayfield, though the remarks which he makes show them to be only approximate. Thus two stations on the coast of Labrador, St. Lewis Bay, in latitude 52° 19′ and longitude 55° 37′, and Henley Island, in latitude 52° 00′ and longitude 55° 53′, give each for the mean of the height of spring and neap tides, 2.3 feet. St. Johns, Newfoundland, gives 5.0 ft. Trepassey Harbor, south of it, 5.8 ft.

Beginning with the southern end of Table A, and following the results northward and eastward, we find from Cape Florida to Savannah and Port Royal a gradual increase of the tides, and then a gradual decrease to Cape Hatteras, with a single contradiction easily explained. Next following the stations on the coast, and omitting those in the bays and sounds, we have a less regular increase to Sandy Hook, and a decrease to Weweeder on Nantucket Island. Next is a less regular regimen, requiring a more detailed examination.

By developing the curved line of the coast into a straight line, and marking upon it the tide stations, which will be thus at nearly their proper distances from each other, and by erecting ordinates at each of the station points, and setting off on a suitable vertical scale the heights of the tides at those points, and connecting the extremities of the several ordinates, we have the broken line shown in diagram A. In drawing this line, the stations of the coast only are joined, and the irregularities are cut off by the curve.

This curve shows distinctly the *physical* division of the coast between Cape Florida and Cape Sable into three great bays. The great southern, from Cape Florida to Cape Hatteras. The great middle, from Cape Hatteras to Siasconsett. The great eastern, from Siasconsett to Cape Sable. Perhaps this latter may be considered as only a portion of a great bay from Siasconsett to Cape Race; but this generalization is at present hardly safe, and I confine myself, therefore, to the more limited view. The tide wave setting into the southern bay rises as the bay contracts; and the heights of the tides along the shores increase as the places are more distant from the chord spanning the entrance.

If we suppose the lines of equal height to be straight lines, and draw them upon the diagram, transferring them to a map of the coast, we shall find that they are more crowded on the more curved side, and

APPROXIMATE COTIDAL LI

SAILING LINES AND LINES OF EQUAL HEIGHT

of the

Atlantic Coast of the United States

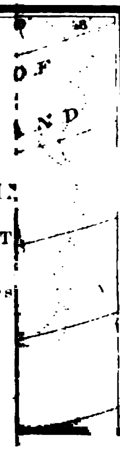
From

investigations in the Coast Survey

A.D.Bache Supd[t]

Scale $\frac{1}{10\,000.000}$

1857



more open on the less curved. The curve indicates Cape Hatteras, and not the inlet which was the tidal station, as the point of least height. The physical cause of this phenomenon is well understood, if it has not yet been reduced to measure.

The next curve shows us plainly the middle bay, having Hatteras for its south-western cape, and Smith's Point, or Weeweeder, for its north-eastern entrance.

The form of the shore is less favorable to regularity, but the result is nevertheless well marked. The interference of tidal waves which takes place off Nantucket tends also in a degree to confuse the results.

The chart shows how simple the system of cotidal lines is in the three bays, running nearly parallel to the shores.

The eastern bay lies between the eastern part of Nantucket (Siasconsett) and Cape Sable, Massachusetts Bay being subsidiary to this.

The tide wave entering the eastern bay follows the deep water, and thus the cotidal lines take generally the directions of the shores, until the tide wave enters the Bay of Fundy. The most probable form of the cotidal lines from XI. to XV. hours inclusive, is shown upon the chart, which is merely an extension of the chart of cotidal lines of the United States coast formerly presented to the Association. The heights increase rapidly from Nantucket to Cape Cod, being 2.2 feet at Siasconsett, and 9.2 feet at Provincetown. At Cape Ann they are nearly of this same height, and increase in passing up and into the bay to 10.0 feet at Boston, and 10.1 feet at Plymouth.

The height at Newburyport is probably local, depending upon the position of the tide gauge. There is but little change from Portsmouth to Portland, and from Cape Sable to Ellenwood's Island. Soon after passing Mt. Desert on the west side, and Ellenwood's Island on the east side, the tide wave has turned into the Bay of Fundy, and the rise increases with extraordinary rapidity.

Shall we look to the greater bay between the Nantucket and Newfoundland Shoals for the cause of the 8 feet rise at Cape Sable, and of the heights from Admiral Bayfield's table? We find the heights along the coast of Nova Scotia to vary from seven to six feet, not with regularity, however. At Cape Breton Island they vary from 6.4 to 4.6 feet, decreasing thus in going northward and eastward. Are these heights due to the crowding of the waters into this greater bay? If so, why are not the heights of Cape Breton greater than those of Nova

Scotia? We require results on the south shores of Newfoundland, and on the Great Bank, to give us clear ideas on these points, and I hesitate to extend the generalization to this tempting field.

The Shoals from Nantucket and broken ground near George's Bank, and the comparatively shoal water in their vicinity on the one side, and the Great Bank of Newfoundland on the other, look as if full of meaning of this sort. Further results may, however, show that this is not the interpretation of the phenomena. The tides of Labrador are but 2.3 feet, bringing us back to the standard of Hatteras, and of Montauk Point, and what probably would be that of Nantucket, but for interferences.

The complicated character of the cotidal lines in this immediate vicinity is indicated by the chart, the lines from XII. to XV. hours being crowded into the very small space of a few miles on the south side of Nantucket.

To return to the more limited scale, within which our inductions are safe, Delaware Bay, New York Bay, Long Island Sound, Narragansett and Buzzard's Bays, Nantucket and the Vineyard Sounds, present, on a smaller scale, the same phenomena of increase in the height of the tide in ascending. On the contrary, in Chesapeake Bay, which widens and changes direction at a right angle immediately from the entrance, the tides diminish in height, as a general rule, in going up the bay.

The results of the heights of tides along the coast are very satisfactorily shown, upon a model which is now before the Association, for superintending the execution of which I am indebted to Mr. Pourtales. The basis is a map of the Atlantic coast from Cape Florida to Cape Race, upon which the cotidal lines of the United States are traced. The tidal stations are marked upon this, and rods cut to length, and proportionate to the rise and fall of the tides at the several stations, are inserted in holes drilled at the station points. The steel rods refer to the heights at exterior stations, and the brass rods to interior ones. Paper cut to the form of the general curve of heights, which has already been explained, and placed behind these rods, serves to show the generalizations with great distinctness.

I propose to call the bay between Cape Florida and Cape Hatteras, the Southern Bay; that between Cape Hatteras and Nantucket, the

Middle Bay; and that between Nantucket and Cape Sable, the Eastern Bay, of the coast of the United States.

The general figure of the coast line has of course heretofore attracted the attention of geographers. The connection with the heights of the tides could only satisfactorily be made out by such a series of tidal observations as those embraced in the Coast Survey.

V. METEOROLOGY.

1. On the Winds of the Western Coast of the United States, from Observations in connection with the U. S. Coast Survey. By. A. D. Bache, Superintendent.

[Communicated by authority of the Treasury Department to the American Association for the Advancement of Science.]

The observations, of which I propose at present to communicate the results, were made in the year 1855, in connection with the tidal observations on the Pacific coast, at three permanent stations — Astoria, San Francisco, and San Diego.

The approximate latitude and longitude of each of the stations are as follows: Astoria, Oregon, lat. 46° 11′ N., long. 123° 49′ W.; San Francisco, California, lat. 37° 48′ N., long. 122° 28′ W.; San Diego, California, lat. 32° 40′ N., long. 117° 12′ W.

The mode of observing was that described in my paper on the winds at Cat Island, read before the Association in 1850. The observers were posted and practised together by Lieutenant W. P. Trowbridge, of the U. S. Corps of Engineers, under whose supervision the observations were made.

The directions of the winds were noted in points, and the force in the conventional scale before referred to. These numbers were reduced to velocity in miles per hour, by the tables given in my former paper, and the quantity of wind blowing from any quarter during a given period was thence readily found.

The tables and diagrams are thus of the same kind as those which I have before presented to the Association. They were made under the direction of Assistant L. F. Pourtales, of the U. S. Coast Survey, to whose care, assiduity, and knowledge I am indebted for the opportunity of presenting them. The computations and diagrams were made by Miss Mary Thomas.

The observations were taken three times each day, at 6 A. M. and P. M., and at noon, except on Monday of each week, when hourly observations took the place of the regular daily ones. From these latter results the reference of the three daily observations to the mean of the day has been made.

The quantities of wind for each hour and for each direction were computed and grouped by months, and then plotted. The eye readily takes in the characteristics of the winds at different periods of the day and year, and for the various directions. To apply these to the reduction of the daily observations, tables were formed of the average time during which each wind blowing would give from observations at the three hours already named the result for the day. For example, the west wind at San Francisco gave for the quantity in twenty-four hours by the daily observations, 505. The mean hourly quantity at 6 A. M. being six, at 12 M., twenty-seven, and at 6 P. M., thirty-one. These quantities respectively, being supposed continued for nine hours, five hours, and ten hours, which agrees with the diagram, would give 499, a number differing but little from the total found for the day. In this way the following table was formed, which was applied to the reductions of the daily observations.

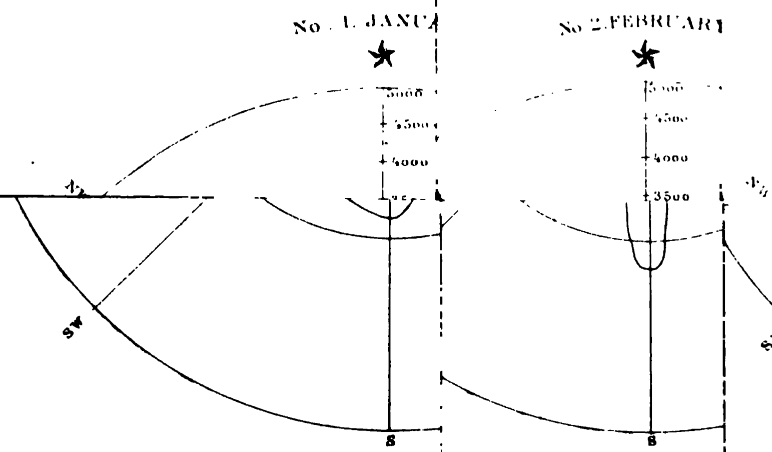
TABLE FOR DEDUCING FROM THE THREE DAILY OBSERVATIONS THE MEAN OF THE DAY.

Astoria.				San Francisco.				San Diego.			
Wind.	6 A. M.	12 M.	6 P. M.	Wind.	6 A. M.	12 M.	6 P. M.	Wind.	6 A. M.	12 M.	6 P. M.
	h.	h.	h.		h.	h.	h.		h.	h.	h.
N. E.	6	6	6	N. & N. E.	9	6	9	N. & N. E.	6	6	6
E. { Oct., Nov., Dec., Jan., Feb., Mar., }	9	6	9	E.	3	3	3	E.	3	3	3
E. { Apr., May, June, July, Aug., Sept. }	3	3	3	S.E., S., & S.W.	9	6	9	S. E. & S.	9	6	9
S. E.	6	6	6	W.	9	5	10	S. W. & W.	18	6	6
S.	9	6	9	N. W.	9	6	9	N. W.	18	7	6
S. W. & W.	9	6	9								
N. W.	8	5	7								
N.	3	3	3								

From the tables of velocities in miles per hour, deduced from the observations by the method just explained, the following table of quantities of wind from different directions for each month is found. The rhumbs are written at the top of the table, the months at the side, and at the meeting point of a vertical and horizontal line from the head and side titles are found the quantities. The last column at the side of the table gives the total quantities for the several months, and below is found the total for each direction for the year.

Quantities of Wind.

	Month.	N.	N. E.	E.	S. E.	S.	S. W.	W.	N. W.	
Astoria.	January	6	795	987	150	324	2,719	1,539		6,520
	February	138	282	1,623	295	3,079	225	850		6,492
	March			1,253	509	933	585	428		3,708
	April	24	180	305	282	1,218	969	1,651	1,801	6,430
	May		102	105	186	536	1,437	876	1,393	4,635
	June		60	33		9	258	2,104	2,760	5,224
	July		150	18			633	700	4,221	5,722
	August		6	12	60	30	279	180	2,543	3,110
	September		54		102	872		246	431	1,705
	October		754	294		1,245	291	123	361	3,068
	November		583	182	399	2,252	2,927	348	447	7,138
	December		1,535	456	693	2,849	138	105	91	5,867
		168	4,501	5,268	2,676	13,347	10,461	9,150	14,048	
San Francisco.	January	2,302	375	147	177	21	1,471	264	1,161	5,918
	February	618	180	45	558	147	992	860	306	3,706
	March	218	60	28	639	470	1,934	2,447	426	6,222
	April	30	72		168	312	2,992	4,845	209	8,628
	May				348	24	1,578	4,928	2,530	9,408
	June					18	8,338	3,428		11,784
	July					9	8,725	2,020	362	11,116
	August						2,608	3,908	2,168	8,684
	September					54	2,588	4,219	500	7,361
	October		6			63	136	4,396	96	4,697
	November	630	252		231	232	508	2,056	222	4,131
	December	414	850	18	652	489	1,290	1,576	234	5,523
		4,212	1,795	238	2,773	1,839	33,160	34,947	8,214	
San Diego.	January	91	147	45	431	813	384	643	987	3,541
	February	204	48	207	1,291	777	688	278	2,461	5,954
	March	284	114	114	1,095	726	912	1,124	3,219	7,588
	April	186	180	36	900	1,019	798	810	2,505	6,434
	May	156	24	57	396	834	1,296	930	2,296	5,989
	June	72	48	66	1,100	827	882	892	2,145	6,032
	July	39	192	16	285	1,320	607	120	4,066	6,645
	August	48	79	24	104	93	595	690	3,823	5,456
	September	48	186	39	54	265	338	426	2,418	3,774
	October	72	156	21	36	132	186	254	2,411	3,268
	November		108	93	240	231	542	198	1,226	2,638
	December	114		24	162	345	686	528	754	2,613
		1,314	1,282	742	6,094	7,382	7,914	6,893	28,311	



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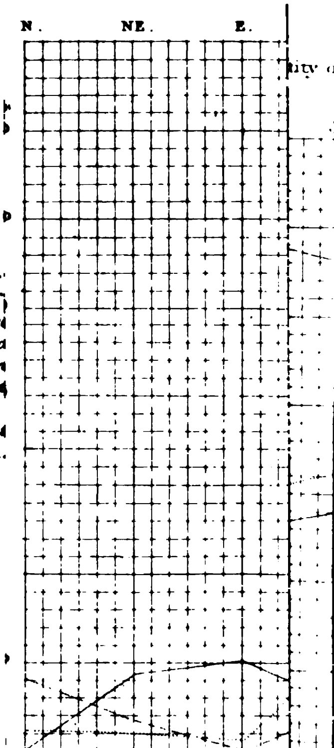

The diagrams plotted on the Compass
direction in every month on a scale of
30000 miles to the inch; for the five
miles to the inch; and for the seven mo
the inch. The circumference of the sm
pass rose represents the zero or start
the year 1855.
The curve for San Diego is represent
do do do San Francisco do...
do do...do Astoria ...... do...

From this table the diagrams representing the quantities for each month from each direction (Plate, Nos. 1 to 12) are taken, and also those showing the total annual quantities of wind from each direction, and the total quantity of wind from all directions for each month. (Plate, Nos. 16 and 17.)

It seems to me altogether probable, from the study of the figures of these tables, that the scale adopted by the observer at San Francisco is greater than that at the other two points. The total quantities at Astoria, San Francisco, and San Diego, are as 59, 87, and 60, and it is hardly probable that there is so large an excess of quantity at San Francisco. I have also the same remark to make as on the observations at Cat Island, on the absence of observations upon the intermediate points between the cardinal ones, showing the tendency to designate the winds only by the cardinal points.

From these diagrams we see at once the simple general regimen of the winds on this coast.

1. The great prevalence of westerly winds, representing a flow of the air at the surface from the ocean in upon the land.

2. The general absence of easterly winds, showing the absence of a return current at the surface.

The proportion of westerly to easterly winds is as 8 to 1.

3. The increase of westerly winds in the summer, and their decrease in the winter.

4. That when easterly winds blow at all, it is, as a rule, during the winter.

5. The N., N. E., and E. winds blow more frequently in the morning than in the afternoon hours.

6. The S. E., S., and S. W. winds are in general pretty equally distributed over the morning and evening hours.

7. The N. W. is the prevailing direction of the ordinary sea-breeze at Astoria and San Diego, and the W. at San Francisco.

Sometimes the W. wind has that character at the first-named stations, and sometimes the S. W. wind at the last named.

A closer inspection of the same diagrams will lead to other interesting results.

Considering the quantities of wind at the three places for the whole year, (Diagram No. 13,) San Diego and Astoria present remarkable similarities. There is more N. E., E. and S. wind at Astoria, and more N. W. wind at San Diego. The axis dividing the area symmetrically is in the same direction. On the contrary, at San Francisco the W. and S. W. winds give the character to the rose, and the axis makes an angle of some sixty-seven degrees with that of the other spaces. All show the same deficiency of easterly winds, and San Francisco is deficient also in southerly ones.

The monthly curves grouped in two periods, from November to March, both included, and from April to October, (Plate, Nos. 14 and 15,) show that the annual curve has the summer type impressed upon it. The summer is, in fact, the windy part of the year. The N. W. wind prevails in August at Astoria and San Diego, and the W. and S. W. at San Francisco.

The scale of Diagram No. 14 is less than that of 15, in the proportion of 10 to 14. There is scarcely any wind from points between North round by East and South. The form of the rose is exceedingly simple, and the generalization very obvious.

The winter system is less simple. The axes for the spaces for Astoria and San Diego make angles of more than 110° with each other. The N. E., E., S., and S. W. winds are considerable at Astoria, and the N. W. wind is deficient. At San Francisco the W. winds give the prominent feature to the rose curve.

As the winter is not the windy season, so the months of March and September are not the windy months; on the contrary, July is one of the windiest months of the year.

San Francisco.

At San Francisco, the great current of air flowing from the sea to the land comes generally from the W. or S. W., rarely from the N. W.

In the period from November to March inclusive, (Diagram No. 14,) the W. is the prevailing wind, exceeding in quantity both the others; the S. W. wind exceeding in quantity the N. W. In the period from April to October, (Diagram No. 15,) the W. and S. W. winds are nearly equal, and each exceeds the N. W.

The W. wind has in general the features attributed to the sea-breeze, beginning after the rising of the sun, increasing until after the hottest part of the day, and dying out, or much diminishing, at nightfall.

The W. and S. W. winds give the prominent features to the wind rose at San Francisco.

The S. W. is the prevailing wind in June and July, S. W. and W. winds blowing nearly the whole of those months, not succeeded by an easterly land breeze, but rising and falling.

The rose curves for May and August resemble each other. The N. W. and S. W. winds being nearly equal in quantity, and each less than the W. wind. So the curves for April and September, when the N. W. wind has nearly died out.

The W. wind diminishes in quantity through March and February, and through October, November, and December, to January.

The N. W. wind increases again from April towards December, and is very small in October and November. The S. W. wind disappears in October, changing the form of the rose curve, but reappearing in November and December, and increasing toward January.

The W. wind has a maximum in April and May, and another in September and October; the minimum being July and January.

The N. wind in December, January, and February, reaching a maximum in January, is the only other point to be noticed. San Francisco partakes with the other places in the general absence of easterly winds. The tables show a little in the winter. There is also but little S. wind there.

Astoria and San Diego.

In general, the winds at these two places resemble each other more than those at San Francisco do either. The rose curves for April, May, June, July, and August (Nos. 4 to 8) have the same general character. The mean curve for the year (No. 18) and for the summer period (No. 15) has also the same general character.

The N. W. wind is the summer wind, and has the characteristics of the sea-breeze, but there is no return land breeze. The N. W. wind reaches a maximum in July, and a minimum in December. It is the great prevailing wind of the year (Diagram, No. 13) at San Diego. As it decreases, it is generally replaced by W. and S. W. winds of less quantity. In December the quantities of the three winds are nearly equal.

The resemblance of these winds at San Diego and Astoria is remarkable, the remarks just made applying generally to both places.

There is, however, much less N. W. wind at Astoria than at San Diego. Except in June, July, and August, there is some S. wind each month at Astoria; and especially from September through October, November, December, and February, it presents a marked feature of the rose.

At San Diego this is less marked, the two agreeing most nearly in quantity in March, April, and May.

The S. E. wind is a distinct feature in both places in February and March, and at San Diego in April and June.

The E. wind is prominent at Astoria in January, February, and March, and the N. E. from October to January inclusive.

Astoria has the most easterly wind, the N. E. beginning in October, and blowing until February, and being replaced by the E. wind in March.

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2. On Ozone. By Prof. Charles Smallwood, of Montreal, C. E.

It would be unbecoming in me, as forming a part of the deputation to Albany last year for the purpose of inviting the Association to meet at this place, were I not to take advantage of the present moment, to greet you, gentlemen Members of the American Association, with a cordial and hearty welcome, and I need scarcely add, that the like sentiment inspires the whole of the inhabitants of this city.

Until the present time, these Annual Meetings have been confined to the United States alone, (although not exclusively American,) and separated only by an imaginary boundary, which has now been removed, for we here meet, united as one family, having one common object in view, "the advancement of science;" we are treading the same peaceful path of knowledge; we are assembled under the broad, the vast, canopy of the American firmament; the gentle breeze that wafts the red cross banner of St. George and merry England alike unfurls the stars and stripes, the emblem of your land of freedom. Long may these two flags entwine in peace, in kindred folds, and may that master-piece of scientific genius, the electric cable, which is at this moment being laid beneath the Atlantic sea, whose waves science has measured with a mighty span, be the peaceful band, that will cement more firmly the destinies of the two great nations of the earth, under the benign and able guidance of your worthy President and our beloved Queen, and may science, which knows no country, no nation, no language, be rendered more subservient to the happiness and welfare of the whole human family.

A year has now passed since the deputation from this place enjoyed the hospitalities of one of your large cities; the familiar and friendly faces of many we met there, and now present, call to mind many pleasant recollections, but as in all things mundane, we have here some cause for sadness, — for in the few fleeting moons that have waned since last we met, death has taken from our midst a Redfield, a Bailey, and a Mitchell, each preëminent in his department of scientific research, and to science and us, an irreparable loss, and the Association has done itself honor in paying a tribute to their memories; but the midnight lamp of the man of science must grow dim, the experimentalist must for ever quit the busy scenes of his laboratory, the eye of the astronomer must be closed, for the life of the philosopher is but mortal.

It is my intention to lay before the section the results of observations made on the amount of ozone present in the atmosphere. The place of observation is at St. Martin's, about 9 miles due west of Montreal, and is 118 feet above the mean level of the sea; it is situated in the centre nearly of the Isle Jesus, an island surrounded by the branches of the Ottawa; the place of observation is a little more than three miles from the river, thus being sufficiently inland to be removed from any transient vapor or fog, which is often present in the proximity of rivers; it is a flat island, and the whole of the neighborhood is under cultivation.

It is not my purpose to enter into a lengthy detail of the chemical composition of ozone; enough for our present purpose to define it to be a compound of oxygen, analogous to the peroxide of hydrogen, or that it is oxygen in an allotropic state, that is, with the capability of immediate and ready action impressed upon it. To Schonbein is awarded the discovery, who, in 1840, applied the term ozone to the peculiar smell which is perceptible during the action of the electrical machine, and also during the decomposition of water by the galvanic apparatus. It was subsequently ascertained that a similar smell is developed by the influence of phosphorus on moist air, and also by a great many chemical changes, and for some time its existence was recognized by its smell, or odor, alone; but in April, 1848, Schonbein became possessed of another of its characters, namely, its oxidizing principle, and it is this property which it possesses more particularly, when we direct our attention to its presence in the atmosphere, although these oxidizing properties may be common to some other bodies, as nitrous acid, which is said to be generated in the atmosphere by atmospheric electricity.

When largely diffused in the atmosphere, it causes, like chlorine, (to which it is somewhat allied,) very unpleasant sensations, such as difficult respiration, and it acts powerfully on the mucous membrane; it kills small animals very quickly; it is insoluble in water, and oxidizes very quickly all metallic bodies; and it has the power, in a large degree, of destroying *miasma* arising from the decomposition of animal and vegetable substances; and Schonbein came to the conclusion, that its formation depended upon the action or formation of atmospheric electricity, and he referred the beneficial effects of thunderstorms to the action of the ozone formed, neutralizing the miasma arising from the decomposition of animal, and may be vegetable, substances; and it possesses, in a powerful degree, bleaching properties, and in this it is again analogous to chlorine.

Since Schonbein brought its properties before the scientific world, it has received more or less attention both from the physician and the meteorologist.

It has been advanced, that during the presence of cholera and other epidemic disease, its absence was remarked; while, on the other hand, when the atmosphere has indicated a great amount present, diseases of the lungs and mucous membrane have been more prevalent; it has been still further stated that its action on the vegetable kingdom is similar in its effects as in the animal economy; the potato disease or rot especially, and other diseases in vegetables have, it is said, been caused by either its absence or presence in too large quantities.

It would far exceed the limits of time allotted to me, to enter fully into the progressive steps of the investigation or history of ozone, for it has engaged the attention of physicians in England and on the continent of Europe, and I am happy to say, that some members of the American Association have devoted considerable attention to it, and I have deemed it of sufficient import to lay before the section the result of some eight years of investigation, or nearly six thousand observations. This includes observations during the visitation of the cholera in 1854, and I heartily trust that the Association may, by its influence, extend these observations through the whole of the United States territory, and, as far as practical, throw some light on its action in the animal and vegetable kingdom; and I am sure a subject of so much importance, and which must (if we are to believe the report of some investigators) exert an influence on both the health of animals and of plants, will be at once a sufficient ground for extending such observations, which should be as uniform as possible.

The method of estimating and detecting the amount of ozone is by what is called the *Ozoneometer*, which is nothing more than slips of paper, wetted with the solution of starch and iodide of potassium; these became blue on exposure, owing to the oxidization of the potassium by the ozone, and the setting free of the iodine; the formula I use, and the one generally adopted, is ʒ i of starch boiled in ℥ i of distilled water, and when cold 10 grains of the iodide of potassium is mixed with it; it is quickly spread on paper and dried in the dark, and must be kept in a dry place and free from light until required, when they are placed in a situation shaded from the sun and rain; these strips are one half an inch wide, and from three to four inches long. Dr. Moffatt, an eminent English physician, and who has paid much attention to the subject, places his slips of paper in a box without a bottom, so as to be *excluded* from the light; but so far as my observations go, I have found so little difference in the two methods, that I have continued that of Schonbein's, as I have before stated, and expose the slips of paper to light, but *excluded from the sun and rain*. The amount of ozone present is estimated in 10ths; the deep shade or saturation being 10, and diminishing in depth of shade to 0.

It has also been asserted that slips of paper placed at high elevations have exhibited a deeper shade. To test this fact I exposed slips of prepared paper at an altitude of 80 feet, on the top of a pole or mast, which is used for collecting atmospheric electricity; and as far as my observations go, I could detect no appreciable difference from those exposed 5 feet from the ground; and if I might be permitted to suggest, that, to insure uniformity, the elevation of 5 feet might be considered the standard height, and which is at once convenient, and far enough removed from the effects of terrestrial radiation or deposit of dew, leaving it, of course, to observers to adopt at the same time any other method which might suggest itself, during the observations on these phenomena.

So far I have, as concisely as the subject would permit, traced its history, properties, and method of observations, and the propriety of so doing may indeed be questionable, before so learned a body; but I have felt that the subject might be new to some present; and with a wish that uniform observations should be made, I deemed it well to state very briefly its prominent character, and in so doing I have thrown myself on your indulgence. I may just state that the color of the test paper may be brought more fully out by moistening it with water.

I shall now proceed to give the *Section* the results of observations made by these means.

The questions for our investigation, and which naturally arise, are these: What is the effect of the presence of ozone on the meteorological conditions of the atmosphere, as indicated by the instruments most in use?

And, secondly, what influence does its presence or absence exert on the health of animals or vegetables? or does its presence or absence give rise to disease?

1. What are the *barometric* indications?

The presence of ozone in the atmosphere is accompanied by a low reading of the barometer, which generally continues while the *ozonic* period lasts; this period is accompanied or terminated almost invariably by precipitation in the shape of rain or snow.

Therometer.—I have observed the presence of ozone at all temperatures when the therometer has indicated 20° (below zero), and as high as 80°, and in all the intermediate temperatures, and it is generally in larger quantities during a fall of snow than of rain. The *psychometer* is a certain indication of the presence of ozone, for it would appear that a moist state of the atmosphere was necessary for its production or development, for when the difference between the *dry* and *wet* bulb therometer is little, the presence of ozone in considerable quantity is invariably present; but when the difference between the two therometers is considerable, no ozone is appreciable by the *ozoneometer*. This fact, and the only one which (as far as my observations here go) is in connection with the presence or absence of ozone, has led me to compare the presence of ozone with the presence of precipitation in the shape of snow or rain, which gives a remarkable coincidence. For in and during the past seven years there were 918 days on which rain or snow fell (this is regardless of the amount or duration), and during the like period there were 816 days on which ozone was present in a quantity of five tenths; any amount below that quantity in this estimation is not taken into consideration in the discussion. In the year

1850	there were	106	days of precipitation,	and	110	days	of ozone.
1851	"	123	"	"	135	"	"
1852	"	136	"	"	152	"	"
1853	"	136	"	"	114	"	"
1854	"	133	"	"	73	"	"
1855	"	140	"	"	110	"	"
• 1856	"	144	"	"	126	"	"

The small amount of ozone in 1854, which was the year of the last visitation of cholera, would tend to favor the opinion that there was a deficiency of ozone in the atmosphere during the prevalence of that epidemic. A deficiency was, however, observed in almost every month of that year, although the number of days on which snow or rain fell were almost equal with the other years; for which see the following table, which shows the amount for each year, and for each respective month: —

YEARS.	1851.		1852.		1853.		1854.		1855.		1856.	
	DAYS OF		DAYS OF		DAYS OF		DAYS OF		DAYS OF		DAYS OF	
MONTHS.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.	Precipitation.	Ozone.
January, . . .	10	5	14	7	9	11	14	6	12	6	12	14
February, . .	6	7	11	8	11	9	16	6	8	9	9	16
March, . . .	11	6	17	11	9	21	17	7	9	6	10	17
April, . . .	12	8	10	11	7	12	10	6	14	8	11	20
May, ...	16	14	9	8	16	12	8	7	6	9	13	20
June, ...	12	12	17	18	15	11	10	8	15	12	10	17
July, ...	13	16	11	17	9	4	5	4	7	11	12	18
August, . . .	8	15	9	16	13	7	7	3	11	12	15	11
September, . .	11	15	10	16	11	9	11	3	12	11	14	12
October, . . .	12	11	17	18	14	5	11	8	18	11	10	8
November, . .	10	13	10	12	14	11	13	11	14	9	15	8
December, . .	11	13	18	14	8	8	12	7	14	6	11	5
Total,	123	135	136	152	136	110	133	73	140	110	144	126

Southerly and easterly *winds* being the point from which our rain or snow generally comes, are for the most part present, during the indications of ozone, while, on the contrary, northerly or westerly winds very rarely accompany its development.

In reviewing these observations, there is no condition of the atmosphere appreciable by our instruments, that indicates the presence of ozone, except the presence of vapor or humidity.

Schonbein has asserted that a high electrical state of the atmosphere was always present when ozone was developed, and that the amount depended essentially on the amount of atmospheric electricity. From the comparison of nearly 6,000 observations on the electrical state of the atmosphere, and the amount of ozone taken at the same hour, at this place, and carefully compared, I have not found that opinion sustained, neither have I found its amount or presence influenced by the appearance of the *aurora borealis*, which has also been said to be the case.

From these observations it would appear that a moist and humid atmosphere was necessary for the development of ozone, and this may account in some measure for its more constant presence and its greater quantity in proximity to the sea. So far as regards its effects on the production of disease in plants, especially the potatoe, and to which it has been more especially referred, it is almost certain that one of two causes must have given rise to the lamentable failure in this useful vegetable; either that the soil must have furnished the medium of disease, or the action of the atmosphere upon the leaves and stem of the plant; — the causes which act upon the stem and leaves involve the action alone of atmospheric influences, while those that act through the medium of the soil are more numerous.

In this neighborhood the disease showed itself after rain followed by a hot sun, the atmosphere being loaded with moisture or vapor, — just the condition essentially proper for indicating the presence of ozone, — and the disease was much more extensive on wet and clayey soils than on sandy or dry ones.

It cannot be doubted that an agent so active as ozone, if really present, must exert a great influence on the health of individuals, as well as animals and plants; the manner of its production, whether by chemical action or electricity or magnetism, demands from us further investigation, and these investigations should be carried out with uniformity for the sake of careful comparison, — one point should not be overlooked, that is, to mark carefully the amount of vapor present in the atmosphere, as the intimate connection between them is too prominent to escape observation.

I have, as you will perceive, offered no theoretical deductions. If, as our continental brethren assert, it does possess such powerful and wonderful properties, it must be evident that the American Association should at once take up the subject, in a way that we may arrive at important conclusions. I should not be justified in expressing a doubt on the labors of others in this department of physical science, neither do I think it fair to offer any conclusions until our observations are more extended; and it is with this intention that I have brought it before the Association, hoping that between now and our next meeting, we may be able to investigate and compare observations so as to give it a proper place in this department of physical investigation.

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3. THE METEOROLOGY OF THE VICINITY OF MONTREAL. By Prof. CHARLES SMALLWOOD, of Montreal, C. E.

I AM well aware that many of you are here for the first time in this, our northern city, and have scanned, and I have no doubt admired, the numerous edifices, — those artificial structures erected by the human hand, guided by human skill, and well suited to our wants. I am also aware that many among you have bent your investigations beneath our alluvial and fruitful soil, to contemplate the geology of our rocky formations, and the deposits of by-gone ages, the work of that Divine Architect at whose command those bright and countless orbs that spangle in our firmament were brought into existence, and which form to the astronomer so many objects for his study; and I felt it might be interesting to you to know something of our climatology, and it is for this purpose I intend laying before the seetion some remarks in illustration, reduced from observations taken at St. Martins, nine miles due west of this place; and I shall for this purpose confine my observations to the means reduced from the last septennial period, although the observations on record extend over a much longer period of time.

The geographical coördinates of the place are 45° 32′ north latitude, and 73° 36′ longitude, west of Greenwich. The cisterns of the barometers are placed 118 feet above the level of the sea. The instruments used are standard instruments; the barometric observations are all reduced to the freezing point (32° F.), and the temperatures are all in Fahrenheit's scale. The hygrometric observations are reduced by the tables and formula adopted at the Greenwich Observatory in England. The receiver of the rain gauge is placed twenty feet above the soil. The direction and velocity of the wind are ascertained by a self-registering instrument, which indicates its velocity by dots on a paper register in miles linear. The electrical apparatus is provided with a collecting lantern, which is elevated eighty feet from the ground. The solar and terrestrial radiators are also read in terms of Fahrenheit's scale. The ozoneometer is of Schonbein's construction. The whole of the means are reduced from three daily observations, taken at 6 A. M., 2 P. M., and 10 P. M.; extra hours are also set apart for any unusual phenomena.

Barometer.—The mean height of the barometer for this period (seven years) was 29.676 inches; the mean reading for the same septennial period in January was 29.744 inches; February, 29.744 inches; March, 29.492 inches; April, 29.679 inches; May, 29.604 inches; June, 29.718 inches; July, 29.715 inches; August, 29.754 inches; September, 29.722 inches; October, 29.619 inches; November, 29.769 inches; December, 29.565 inches. The highest reading observed and on record here was on the 8th of January, 1855, and at 4 P. M. it attained the unusual height of 30.876 inches; the lowest reading on record was in December, also in 1855, and was 28.689 inches, giving an absolute range of 2.187 inches. The mean yearly range for the seven years was 1.032 inches, and for the months as follows:—

	Inches		Inches		Inches
January	1.550	May	0.800	September	0.815
February	1.131	June	0.752	October	0.951
March	1.145	July	0.616	November	1.295
April	1.090	August	0.701	December	1.538

There are two maxima and two minima variations occurring in the barometer in the 24 hours; the maxima variations occur at between 9 and 10 o'clock A. M., and between 9 and 10 P. M.; the minima variations occur at 3 A. M. and 3 P. M.

Thermometer.—The temperature of the air for the same period (7 years) exhibits a yearly mean of 41° 56′. The mean temperature of January was 13° 26′; February, 13° 31′; March, 25° 44′; April, 40° 12′; May, 55° 70′; June, 62° 11′; July, 74° 78′; August, 61° 21′; September, 58° 12′; October, 46° 04′; November, 31° 49′; December, 13° 80′. The absolute mean range for the same period has been from 90° 9′ + to 27° 4′ — (below zero). The absolute monthly range was, in

January	+ 40° 7 to 25° 1 —	July	+ 97° 1 to 47° 8 +
February	+ 41° 1 to 25° 2 —	August	+ 96° 7 to 40° 6 +
March	+ 56° 0 to 6° 7 —	September	+ 91° 2 to 30° 4 +
April	+ 75° 6 to 10° 1 +	October	+ 75° 7 to 23° 8 +
May	+ 86° 6 to 25° 7 +	November	+ 60° 4 to 5° 7 +
June	+ 94° 5 to 40° 5 +	December	+ 42° 1 to 26° 3 —

The highest temperature in the shade on record here was 100° 1′, and the lowest was 36° 2′ below zero; giving a climatic range of 136° 3′. The hottest month is July, and the coldest month is February. The warmest part of the day in summer is at 3 P. M., and in the winter season at 2 P. M. The coldest part of the day in winter is at a little before sunrise.

The mean yearly temperature of the *dew point* reduced for the same period, was 35° 6′, and for the different months as follows:—

January	9° 6	July	65° 0
February	7° 4	August	53° 1
March	20° 2	September	52° 2
April	34° 6	October	40° 8
May	47° 2	November	26° 1
June	54° 1	December	8° 1

The relative degree of humidity for that period, saturation being 1.000, was 814; and for the months:—

January	.869	July	.744
February	.808	August	.765
March	.835	September	.809
April	.812	October	.821
May	.774	November	.824
June	.770	December	.832

The *elastic force of vapor* exhibits a daily maximum at 3 A. M., and a minimum at between 3 and 4 P. M. The summer quarter, which embraces June, July, and August, is the dryest quarter; next is the spring quarter, which embraces March, April, and May. The autumnal and winter quarters are the most humid. Complete saturation does not often occur; it has nevertheless taken place about four or five times in each year.

The mean number of days on which *rain* fell for the same period is 73 per year, and the number of days on which *snow* fell is 43, making a sum of 116 days on which precipitation took place, leaving 249 fair days as a yearly mean for the 7 years. There is, on an average, about 110 nights suitable for astronomical purposes in each year.

The yearly mean amount of rain for the same period was 43.004 inches in depth on the surface; and the depth of snow also on the surface, shows a yearly mean of 95.76 inches. The monthly means for snow and rain are as follows:—

	Inches of Rain.	Inches of Snow.		Inches of Rain.	Inches of Snow.
January	0.600	22.38	July	3.003	
February	0.167	25.00	August	5.908	
March	0.380	18.79	September	5.831	
April	4.624	2.46	October	6.063	1.80
May	4.386		November	5.056	4.34
June	6.013		December	0.940	17.71

This gives a mean of 52.380 inches of rain and melted snow. This is reduced by the Smithsonian formula, which does not hold good or correct for low temperatures; and I think "1 to 8" would be more accurate. The greatest amount of rain, which fell in 24 hours, on record here, was in September, 1853, and amounted to 5.142 inches, but this is unusual. You will perceive that we are a little more than 5 months without snow.

The difficulty in this climate of measuring the amount of evaporation from the surface of the water, except for 7 months of the year, owing to frosty nights, has induced me to undertake the registration of the amount of evaporation from the surface of ice during the remainder of the year, (5 months,) so as to compensate in some measure for the defect in the observations on the amount of evaporation from the watery surface. These combined observations give a mean of more than 30 inches as the amount of water evaporated. The evaporator is shaded from the sun and rain, but is exposed to the currents of wind; so is also the icy surface in winter.

I am led to believe this amount is tolerably correct. The mean amount of evaporation from the surface of water alone for the 7 months is nearly 21 inches, the remaining amount being furnished by the evaporation which takes place from the surface of ice during the remaining 5 months.

Winds. — The most prevailing wind of the year is the westerly, and the mean direction for the 7 years in the different months is as follows: —

January	N. E. by E.	July	S. W. by W.
February	W. S. W.	August	W. N. W.
March	W.	September	W. N. W.
April	N. E. by E.	October	W. by W.
May	N. W. by N.	November	W. N. W.
June	S. W. by W.	December	N. E. by E.

The greatest velocity on record here exceeds somewhat 60 miles per hour linear. There seems a disposition for a change both in the direction and velocity at 3 P. M. and at 3 A. M., which corresponds precisely with the diurnal barometric fluctuations. The whole amount of miles linear of wind during the past year (1856) was 53,061.63 miles, which being resolved into the four cardinal points, gave N. 6,969.80 miles; S. 5,298.89 miles; E. 10,776.40 miles; and W. 30,016.56 miles. The maximum velocity during the past year was 44.40 miles per hour. There were 2,220 hours 15 minutes calm, and 6,546 hours during which the atmosphere was in motion. Below is a table of the anemometric observation during the year 1856, showing the direction and amount of miles from each quarter of the compass, and also the amount of miles run in each month; also the amount of calm in hours for each month.

Course.	Velocity in Miles.	Course.	Velocity in Miles.	Course.	Velocity in Miles.
N.	310.50	S. E. by E.	403.00	W. S. W.	4679.66
N. by E.	211.50	S. E.	297.00	W. by S.	4542.50
N. N. E.	412.00	S. E. by S.	690.20	W.	3111.80
N. E. by W.	661.70	S. S. E.	374.00	W. by N.	3103.00
N. E.	1325.00	S. by E.	578.50	W. N. W.	4790.00
N. E. by E.	8092.60	S.	714.70	N. W. by W.	2112.80
E. N. E.	892.70	S. by W.	238.30	N. W.	2728.00
E. by N.	237.10	S. S. W.	497.57	N. W. by N.	1269.00
E.	86.30	S. W. by S.	608.10	N. N. W.	687.00
E. by S.	156.00	S. W.	2375.70	N. by W.	77.00
E. S. E.	240.00	S. W. by W.	3845.60		

RESOLVED INTO THE FOUR CARDINAL POINTS.

Months.	Miles North.	Miles South.	Miles West.	Miles East.	Total Miles.	Hours and Minutes Calm.
January	395.40	95.77	4115.66	1744.10	6351.23	143.00
February	71.90	280.00	4854.80	277.20	5463.90	166.00
March	674.80	917.30	3706.60	567.70	5866.40	177.00
April	234.00	116.00	1644.00	2585.10	4579.10	247.00
May	1415.00	484.00	1323.00	1321.00	4540.00	179.10
June	350.00	768.00	1450.00	582.00	3130.00	168.40
July	776.00	345.00	1652.20	111.00	2884.00	174.20
August	621.00	242.30	1018.20	569.30	2450.00	269.20
September	471.00	589.50	1249.00	490.00	2799.50	243.14
October	843.00	371.00	2270.00	248.00	3752.10	226.45
November	653.00	650.00	2386.00	975.00	4644.00	149.00
December	464.70	458.00	4387.00	1310.00	6628.20	78.30

The yearly mean intensity of the sun's rays for the same septennial period, is 102° 6′, and for the months as follows:—

January	79° 4	July	121° 4
February	87° 5	August	118° 4
March	119° 4	September	103° 9
April	107° 1	October	99° 4
May	110° 5	November	89° 7
June	110° 2	December	84° 9

The yearly (septennial) mean of terrestrial radiation was 11° 6′, and for the months as follows:—

January	20° 9	July	46° 7
February	22° 6	August	38° 1
March	18° 2	September	34° 2
April	8° 0	October	18° 9
May	29° 6	November	11° 6
June	39° 1	December	25° 1

The amount of dew is very variable, but bears a proportion to the degree of terrestrial radiation.

The mean of cloudless days were 57 days perfectly cloudless; the prevailing clouds are the cumuli stratus and cirri stratus.

The song sparrow (*fringilla melodia*), the harbinger of the Canadian spring, generally makes its first appearance the first week of April. Frogs (*rana*) are first heard about the 23d of April. Shad (*alosa*) are caught the last week in May. Fire-flies (*lampyrus coruscа*) are first seen about the 24th of June; and the snow bird (*plectrophanes nivalis*) generally makes its first appearance about the 20th of November. Swallows (*hirudo rufa*) about the 18th of April. Our winter generally sets in about the latter week of November, or the first week in December, and is ushered in by a fall of snow from the N. E. by E., and this is the point from which our winter storms come. Rain generally comes accompanied with a wind from the S. S. W., or S. E., and also from the N. E. by E.

We have generally a few days of that poetic season, the Indian summer, in November.

> "The year's last, loveliest smile,
> That comes to fill with hope the human heart;
> And strengthen it to bear the storms awhile,
> Till winter's days depart."

Our snow storms of winter are from the N. E. by E.; and for some hours before they form, the eastern horizon becomes gradually covered with heavy *strata* clouds of a deep leaden hue. The upper strata of clouds are generally a mixture of *cirri cumulus* and *stratus*, moving from the south; but the surface wind is from the point I have stated, N. E. by E. The wind during these storms often attains a velocity of some 30 or 40 miles per hour; the barometer is falling, and the thermometer somewhere about zero, the psychometer indicates an increasing amount of moisture, the electrometers indicate a very high tension of *negative* electricity, often an amount of 300° in terms of Volta's No. 1 electrometer, and sparks are constantly passing between the receiver and discharger for hours.

Minute but perfect crystalline forms of snow commence to fall, and may continue for some 48 hours; and I have seen some 12 or more inches of snow fall during this time. Precipitation then ceases; the wind veers *always* by the N. to the W., or W. N. W., with a velocity of some 30 miles per hour, (this is our cold term) ; and the wind carries the loose, finely crystallized snow in clouds before it; this is, in Canadian parlance, a "poudrerie." The wind is intensely cold; the thermometer, during this period, attains a minimum of some 30° below zero. The sky is partly covered by *cumuli* clouds, with a few *strati*; the electrometers still indicate a high tension, but of an opposite or *positive* character. This westerly wind may last some 48 hours or more, and lulls down at sunset; may be, of the second day, into a calm. The blue tint of the sky is very deep, and the rays of the setting sun throw a red or orange shade on the snowy scene, and the atmosphere attains a greater dryness, and the electrical action gradually ceases with the wind.

Our thunderstorms of summer, which give a yearly mean of 14 (for the same period of 7 years) are of short duration, forming generally in the W. or N. W., and the electricity varies in kind.

The months of April, May, and June bring returning summer; the nights of July and part of August are generally oppressive; the temperature often remains at 70° during the night; but the Canadian autumn is very pleasant. The woods, with its leaves of a thousand varied tints, and the blue and cloudless sky, with frosty nights, remind us that the good times of the merry sleigh-bells are near.

Notwithstanding these vicissitudes and extremes of temperature, the soil is very productive, and vegetation prolific and rapid; and it has again pleased an all-wise Providence, during the present year, to crown the labors of the Canadian husbandman with a bountiful and abundant harvest.

PART II.

B. NATURAL HISTORY.

I. GEOLOGY AND GEOGRAPHY.

1. EXPLANATIONS OF THE GEOLOGICAL MAP OF MISSOURI, AND A SECTION OF ITS ROCKS. By PROFESSOR G. C. SWALLOW, State Geologist of Missouri.

IN calling your attention to the map and section of the rocks of Missouri, it has been deemed advisable to give a brief description of the formations, and the useful minerals, which they contain.

So far as observed, the stratified rocks of Missouri belong to the following divisions: —

SYSTEM I. — QUATERNARY.
SYSTEM II. — TERTIARY.
SYSTEM III. — CRETACEOUS.
SYSTEM IV. — CARBONIFEROUS.
SYSTEM V. — DEVONIAN.
SYSTEM VI. — SILURIAN.

The rocks of these divisions will be examined in their order, from the top, down.

SYSTEM I. — QUATERNARY.

As the formations of this system have been described in another paper, I shall simply give their names here, in their order, as follows: —

ALLUVIUM.	BOTTOM PRAIRIE.
BLUFF.	DRIFT.

SYSTEM II. — TERTIARY.

There is a formation made up of clays and sands, extending along the bluffs, and skirting the bottoms, from Commerce, in Scott county, westward to Stoddard, and thence south to the Chalk Bluffs in Arkansas.

The following section, obtained in the neighborhood of Commerce, will give a good idea of the character of these beds. The strata are numbered in their natural order, from the top, down.

No. 1. 9 feet. Pebbles, sand and clay, intermingled.

No. 2. 2 feet. Sand and iron ore, brown haematite.

No. 3. 10 feet. Brown and buff sand interstratified.

No. 4. 12 feet. Buff and white sand interstratified, containing rounded masses of sandstone of the same character and color as the sand forming the strata.

No. 5. 5 feet. Clay and gravel, of a bright chrome yellow.

No. 6. 1 foot. Clay and haematite ore, nearly all iron.

No. 7. 47 feet. Blue shale, which separates, on exposure, into rhomboidal masses.

No. 8. 2 feet. Carbonate of iron in septaria and nodular masses, or in regular strata, which break into rhomboidal masses.

No. 9. 6 feet. Blue shale, like No. 7.

No. 10. 1 foot. Iron ore, like No. 8.

No. 11. 11 feet. Blue shale, like No. 7.

No. 12. 1½ feet. Carbonate of iron, like No. 8.

No. 13. 31 feet. Blue shale, like No. 7, with some thin bands and nodules of iron ore.

No. 14. 7 feet. Sandy clay, with thin strata, and globular masses of haematite ore.

No. 15. 18 feet. White sand, interstratified with thin brown strata, containing some rounded masses of sandstone.

No. 16. 5 feet. Sand, of a light peach-blossom color, interstratified with brown beds.

No. 17. 12 feet. White sandstone, in thick beds. The upper part is hard and vitreous; but the lower is soft and friable. This rock very much resembles the Saccharoidal sandstone of the

Calciferous series, and appears to have been much worn by running water.

No. 18. $\frac{1}{12}$ feet. Very hard, compact, oxide of iron. It is strong, and rings like pot-metal.

No. 19. 20 feet. Salmon-colored, white, purple, and yellow sands, interstratified with clays of the same colors.

No. 20. 1 foot. Spathic iron ore.

No. 21. 13 feet. Blue potter's clay. — 214 feet, total thickness.

I have observed no fossils in these beds, except the impression of a leaf on the sandstone of No. 17.

In the absence of any positive proof of the age of these interesting strata, I have marked them Tertiary, until future discoveries shall show their true position.

The *iron ore* of these beds is very abundant, and exceedingly valuable. The Spathic ore has been found in no other locality in south-eastern Missouri, so that the large quantity and excellent quality of these beds will render them very valuable for the various purposes to which this ore is peculiarly adapted.

The *white sand* of these beds will be very valuable for glass-making, and for the composition of mortars and cements. The *clays* are well adapted to the manufacture of pottery and stoneware.

System III. — Cretaceous.

Beneath the Tertiary beds above described, in the bluffs of the Mississippi above Commerce, the following strata were observed: —

No. 1. 13 feet. Argillaceous sandstone, variegated with gray, brown, and white.

No. 2. 20 feet. Soft bluish-brown sandy slate, containing large quantities of iron pyrites.

No. 3. 25 feet. Whitish-brown impure sandstone, banded with purple and pink.

No. 4. 45 feet. Slate, like No. 2.

No. 5. 45 feet. Fine white silicious clay, interstratified with white flint more or less spotted, and banded with pink and purple.

No. 6. 10 feet. Purple, red, and blue clays. — 158 feet, the entire thickness.

These beds are very much disturbed, fractured, upheaved, and tilted, so as to form various faults and axes, anticlinal and synclinal; while the strata, above described as Tertiary, are in their natural position, and rest nonformably upon these beds.

These facts show the occurrence of great disturbances, subsequent to the deposition of these beds, and anterior to the formation of the strata above.

We have no clue to the age of these rocks, save that they are older than the Tertiary beds above, and newer than the Trenton limestone below. They somewhat resemble some Cretaceous beds found in several places on this part of the continent; and these facts have led me to the inquiry, whether they are Cretaceous. Our future investigations may show their true position.

We have observed no fossils in these rocks.

System IV. — Carboniferous.

This system presents two important divisions:—

Upper Carboniferous, or Coal-Measures.
Lower Carboniferous, or Mountain Limestone.

The Coal-Measures are made up of numerous strata of sandstones, limestones, shales, clays, marls, spathic iron ores, and coals. We have observed about 1,500 feet of these coal-measures, containing numerous beds of iron ore, and at least eight or ten beds of good workable coal.

These rocks, with the accompanying beds of coal and iron, cover an area of more than 27,000 square miles in Missouri.* If a line be drawn from the north-eastern corner of the State, through Clark, Lewis, Shelby, Monroe, Andrain, Callaway, Boone, Cooper, Saline, Henry, St. Clair, Cedar, and Dade counties, to the middle of the western boundary of Jasper, this irregular boundary will separate the great body of the coal-measures, on the north-west, from the older rocks, on

* The Missouri coal basin is one of the largest in the known world. Besides the 27,000 square miles in Missouri, there are in Nebraska at least 10,000 square miles, in Kansas, 23,000, in Iowa, according to Dr. Owen, 20,000, in Illinois, 20,000 (?), making, in all, at least 100,000 square miles. And we may expect the explorations of Maj. Hawn in Kansas, and others in Nebraska, will add much more.

the south-east. Besides the large body of coal-measures on the north-west side of this line, there are extensive beds in Cole, Moniteau, St. Charles, St. Louis, and Callaway counties. The common bituminous and cannel coals are the only varieties of this mineral observed. These exist in vast quantities — one might almost say, inexhaustible.

The fossils are numerous and interesting. So far as our observations extend in Missouri, the *Fusulina cylindrica*, *Spirifer Meusebachanus*, *S. planoconvexa*, *S. hemiplicata*, *S. Kentuckensis*, *Productus splendens*, *P. aequicostatus*, *P. Nebrascencis*, *P. Wabashensis*, *Chonetes mesoloba*, *C. parva*, *C. Smithi*, *Myalina subquadrata*, *Allorisma regularis*, *A. terminalis*, *Leda arata*, *Pleurotomaria sphaerulata*, *Campophyllum torquium*, and *Chaetetes milleporaceus*, are confined to, and very characteristic of, the coal-measures.* The discovery of the fact that these fossils are confined to the coal-measures, has enabled us to point out the existence of the coal-measures, and the coal beds contained in them, over an area of many thousand miles, where geologists had supposed no coal-measures and no coal existed.

Of the Lower Carboniferous rock, we have observed the following formations: —

Chester Sandstone	75 feet.
Upper Archimedes Limestone	250 "
Ferruginous Sandstone	195 "
St. Louis Limestone	225 "
Archimedes Limestone	350 "
Encrinital Limestone	550 "

It will be observed, that the *Chester Sandstone* and the *Upper Archimedes Limestone* have been added since the publication of our Second Annual Report, in 1854. After making a careful comparison of a large collection of fossils from Chester, with those from the northern

* So far as I know, the Second Annual Report of the Missouri Survey made known the very striking and important difference between the fossils of the coal-measures and the Lower Carboniferous rocks of the Mississippi valley. It was also shown by the same report that some, at least, of the different beds of limestone in the coal-measures could be distinguished by their fossils. This is another application of the use of fossils, of vast importance in tracing out the position of the various coal beds in these rocks.

part of the State and Iowa, I am fully satisfied that the beds at Chester, in Illinois, and St. Mary's, in Missouri, are distinct from the Archimedes beds in the northern part of the State, and at Keokuk.

I had had some doubts as to the true position of these beds in the series; but the fact that a large number of the fossils are so similar to some found in the more northern beds, and at the same time so different from any thing found in any of the other Carboniferous rocks, and also the fact, that soon eight or ten of the Chester species appear to be identical with those found in the northern beds, led me to place these rocks in the same formation with the Lower Archimedes beds.

But, on the authority of Prof. Hall and Dr. Norwood, who have had a better opportunity of examining these rocks and their fossils, I now place them above the Ferruginous sandstone, as in his section. The sandstone above these beds is also placed in this series; for, so far as my observations extend, it has much stronger affinities with the sandstones in the Lower Carboniferous series than with those in the coal-measures. The following is the best section I have been able to obtain of these rocks.

No. 1. 65 feet of a heavy bedded, irregularly stratified, brownish sandstone. It is made up of round and angular pellucid particles, having the interstices filled with a fine, opaque, brown substance, which is often replaced by oxides of iron and manganese.

No. 2. 90 feet of limestone, with thin, shaly strata between the beds. The upper beds are usually thin, light gray, coarsely crystalline, and brittle; but the lower beds are thick, more compact, and of a bluish gray color.

No. 3. 58 feet of dark blue shales, with gray and purple beds intercalated.

No. 4. 9 feet. Bluish gray, coarse grained, and thin bedded limestone, interstratified with blue shale.

No. 5. 20 feet. Sandstone. The upper is in thin beds, which are ripple-marked and fossiliferous. The lower part often exhibits well marked, irregular lines of deposition, but no distinct lines of stratification. This sandstone is sometimes replaced by blue and sandy shales.

No. 6. 80 feet. Bluish gray, crystalline limestone, interstratified with blue shales.

No. 7. 10 feet. Light gray, fine ground limestone.

These beds contain a species of *Fenestella* (Archimedes), with a strong axis like one found in northern and central Missouri, similar to *F. Worthenii*, of Hall.

In this formation, we have observed the following fossils: *Productus cora*, *P. elegans*, *Spirifer Leidyi*, *S. incrassatus* (?), *S. spinosus*, *S. lineatus* (?), *Spirigera hirsuta*, *Athyris subtilita*, *Atrypa serpentina*, *Orthis umbraculum* (?), *Fenestella lyra*, *F. Swallvana*, *F. Meekana*, *Pentremites pyriformis*, *P. sulcatus*, *Agassizocrinus dactyliformis*, and *Poteriocrinus occidentalis*.

The *Ferruginous Sandstone* is variable in its lithological characters. In some places it is very white and Saccharoidal; in others, fine, impure particles are disseminated through the mass, and the color becomes a dirty brown; and in a few localities, as near Fulton, Callaway county, it is a coarse conglomerate. But generally, where well developed, it is a coarse-grained, heavy-bedded, friable sandstone, colored with various shades of brown, red, and purple, as it appears in the bluffs near Salt Creek Sulphur Springs, some two miles west of Osceola; or clouded with yellow and red, as on Turkey creek, in Cedar county. The upper part is more regularly stratified and finer grained, contains more argillocalcareous matter, and has a light brown, yellowish gray, or cream color. It is very soft when quarried, and may then be dressed for building purposes; but exposure renders it much harder and more durable.

This sandstone contains large quantities of oxides of iron, brown and red hematites, which, in many places, form extensive beds of excellent ore. In Cooper county, this sandstone contains a good bed of ore, three feet thick. The same bed again shows itself in several places in Sec. 33, Town. 48, R. 19, and in various other places in the county. It was also observed, in large masses, on Grand river, in Henry county; in Sec. 28, Town. 39, R. 24, in St. Clair county; and in Bates and Hickory, and in still greater abundance, in the western part of Greene county.

The large quantities of iron in this sandstone have led me to give it the provisional name, *Ferruginous Sandstone*. It is found skirting the eastern border of the coal-measures, from the mouth of the Des Moines to McDonald county.

The *St. Louis Limestone* is made up of hard crystalline, and compact, gray and blue, somewhat cherty limestones, interstratified with thin partings of blue shale. Its stratigraphical position is between the Ferruginous sandstone and the Archimedes limestone, as seen near the Des Moines, and near the first tunnel on the Pacific railroad. It is found in Clark and Lewis counties, but attains its greatest development in St. Louis, from which the name is derived.

The most characteristic fossils yet described, are *Palaechinus multipora, Lithostrotion Canadense, Echinocrinus Nerci, Poteriocrinus longidactylus*, and *Atrypa lingulata.*

The *Archimedes Limestone.* In this formation are included the "*Arenaceous bed*," the "*Warsaw* on *Second Archimedes Limestone*," the "*Magnesian Limestone*," "*Geode bed*," and the "*Keokuk* or *Lower Archimedes Limestone*" of Prof. Hall's section, and the lead-bearing rocks of south-western Missouri, which, though different from any of the above beds, are more nearly allied to them than to the Encrinital limestone below. All of the above beds are easily recognized in Missouri, save, perhaps, the Warsaw limestone, which is but imperfectly represented in our north-eastern counties, where the "Keokuk limestone," the "Geode beds," and the "Magnesian limestone," are well developed.

The most characteristic fossils described, are *Fenestella Worthenii* (?), *F. Owenanæ, Agaricocrinus tuberosus, Actinocrinus Humboldtii, Spirifer incrassatus* (?).

This formation extends from the north-eastern part of the State, to the south-west, in an irregular zone, skirting the eastern border of Ferruginous sandstone. The extensive and rich lead deposits of south-western Missouri are in this formation. These mines occupy an area of more than one hundred square miles, in the counties of Jasper and Newton.

The *Encrinital Limestone* is at once the most extensive and best characterized of the divisions of the Carboniferous limestone. It is made up of brown, buff, gray and white, coarse, crystalline, heavy-bedded limestones. The darker colored, impure varieties, prevail near the base, while the lighter, and more purely calcareous, strata abound in the upper part. It everywhere contains globular, ovoid, and lenticular masses of chert, disseminated or arranged in beds, parallel to the lines of stratification. These masses of chert are more abundant in the upper beds; in fact, the upper beds are made up almost exclusively of this mineral. The strata of this formation are frequently intersected by joints, resembling the sutures of the cranium. The remains of corals and mollusks are very abundant; some of the strata are made up, almost entirely, of their exuviæ, especially of the joints and plates of *Crinoideans*. In the south-west, these strata rest upon some seventy or eighty feet of hard, porous, and thick-bedded silicious rock, which are included in this formation, as they have more affinities with it than with the Chemung below. There are nine divisions of this formation in Missouri, which are quite well marked by their fossils and lithological characters.

The Encrinital limestone extends from Marion county to Greene, forming an irregular zone on the east of the Archimedes beds.

*Chemung.**

This group presents three formations, very distinct in lithological characters and fossil remains. They have received the following provisional names:—

CHOUTEAU LIMESTONE	85 feet.
VERMICULAR SANDSTONE AND SHALES	75 "
LITHOGRAPHIC LIMESTONE	125 "

The *Chouteau Limestone*, when fully developed, is made up of two very distinct divisions.

1. At the top, immediately under the Encrinital limestone, we find some forty or fifty feet of brownish gray, earthy, silico-magnesian limestone, in thick beds, which contain disseminated masses of white or limpid calcareous spar. This rock is very uniform in character, and contains but few fossils. Reticulated corals, and Fucoidal markings, like the *Cauda-galli*, are most abundant. In the quarry, it is quite soft, but becomes very hard on exposure, and forms a very firm and durable building rock. It is also hydraulic, and forms a good cement.

* There is some difference of opinion respecting the system to which this group belongs; but if we make a division of the Missouri rocks into Devonian and Carboniferous, the line of separation, the most distinctly marked, is between the Encrinital and Chouteau limestones.

2. The upper division passes down into a fine, compact blue, or drab, thin-bedded limestone, whose strata are quite irregular and broken. Its fracture is conchoidal, and its structure, somewhat concretionary. Some of the beds are filled with a great profusion of most beautiful fossils. In many, the organic substance has been replaced by a calcareous spar. The most characteristic are *Spirifer Marionensis, Productus Murchisonianus, Chonetes ornata, Atrypagregaria, A. occidentalis, A. obscuroplicata, Leptaena depressa, Avicula Cooperenensis, Mytilus elongatus*, and several new species of *Trilobites*.

Chouteau Limestone has been applied to these rocks, as they were well developed at the Chouteau springs, in Cooper, where I first found large quantities of its new, beautiful, and characteristic fossils.

In the north-eastern part of the State, the Chouteau limestone is represented by a few feet of coarse, earthy, crystalline, calcareous rock, like the lower division of the Encrinital limestone, as there developed. There is, indeed, in that part of the State, no change of lithological characters as you pass from the Encrinital limestone to this formation; but the change in the organic remains is both sudden and great.

The Vermicular Sandstone and Shales. The upper part of this formation is usually a buff, or yellowish brown, fine-grained, pulverulent, argillo-calcareous sandstone. It is usually perforated in all directions with pores, filled with the same materials more highly colored, and less indurated. This portion, when exposed to atmospheric agencies, often disintegrates, and leaves the rock full of winding passages, as if it were worm-eaten. In the south-west, the harder part is much more silicious and indurated. The middle portion is a bluish-brown and gray silico-calcareous, magnesian shale. It has a conchoidal fracture, — the peculiar markings of the upper part, — together with those of a curious undescribed *Fucoid.* The lower part is usually a blue, sometimes brown, argillaceous shale, or fire-clay, in regular thin strata.

This formation contains but few fossils, and those are in the upper portions. *Spirifer Marionensis, Productus Murchisonianus, Chonetes ornata, Avicula circula*, the *Fucoids*, above named, and the *cauda-galli*, are most numerous. These beds can always be detected by the lithological characters and its peculiar *Fucoids*.

The *Lithographic Limestone* is a pure, fine, compact, even-textured silicious limestone, breaking rather easily, with a conchoidal fracture, into sharp, angular fragments. Its color varies from a light drab to the lighter shades of buff and blue. It gives a sharp, ringing sound under the hammer, from which it is called "pot-metal," in some parts of the State. It is regularly stratified in beds varying from two to sixteen inches in thickness, often presenting, in mural bluffs, all the regularity of masonry, as at Louisiana, on the Mississippi. The beds are intersected by numerous fractures, leaving surfaces covered with beautiful dendritic markings of oxide of iron. The strata are much thinner towards the top, where they often become silicious, and sometimes pass into an impure, thin-bedded, oolitic limestone, as in the bluffs, south-east of Elk Spring, in Pike county.

It has but few fossils. The most abundant are *Spirifer Marionensis*, *S. cuspidatus*, *Productus Murchisonianus*, *P. minutus*, *Proteus Missouriensis*, *Filictes gracilis*, a *Conularia*, *Fucoides cauda-galli* (?), and several large-chambered shells.

The Chemung rocks extend from Marion county to Greene, along the eastern border of the Carboniferous strata.

System V. — Devonian.

The Devonian system of Missouri contains the three following formations: — Hamilton Group, Onondaga Limestone, Oriskany Sandstone.

The *Hamilton Group* is made up of some forty feet of blue shales, and 130 feet semi-crystalline limestone, containing *Dalmania calliteles*, *Phacops bufo*, *Spirifer mucronatus*, *S. sculptilis*, *S. congesta*, *Chonetes carinata*, *Favosites Cosaltica*.

Onondaga Limestone. This formation is usually a coarse gray, or buff, crystalline, thick-bedded, and cherty limestone, abounding in *Terebratula reticularis*, *Orthis resupinata*, *Chonetes nana*, *Productus subaculeatus*, *Spirifer euruteines* (Owen), *Phacops bufo*, *Cyathophyllum rugosum*, *Emmorisia hemispherica*, and a *Pentamerus*, like *galeatus*.

No formation in Missouri presents such variable and widely different lithological characters as the Onondaga. It is, generally, a coarse, gray, crystalline limestone; often, a somewhat compact, bluish concretionary limestone, containing cavities filled with green matter, or calcspar; in a few places, a white Saccharoidal sandstone; in two or three localities, a soft, brown sandstone, and, at Louisiana, a pure white oolite. Will those who would have us follow lithological characters exclusively tell us how we are to identify this formation, without its fossils, at these various localities?

The *Oriskany Sandstone* of Missouri is a light-gray *limestone*, which contains the *Spirifer arenosa*, *Leptæna depressa*, and several new species of *Spirifer*, *Chonetes*, *Illaenus*, *and Lichas*.

The Devonian rocks occupy a small area in Marion, Ralls, Pike, Callaway, Saline, and Perry counties.

System VI. — Silurian.

Of the *Upper Silurian* series, we have the following formations: —

Lower Helderberg	350 feet.
Niagara Group	75 "
Cape Girardeau Limestone	60 "

The *Lower Helderberg Group* is made up of buff, gray, and reddish cherty, and argillaceous limestones, blue shales, and dark graptolite slates, *Dalmania tridentifera*, *Cheirurus Missouriensis*, *Encrinurus punctatus* (?), *Calymene rugosa*, *Orthis hybrida*, *O. elegantula*, and several species of *Platyostoma*, are the prevailing fossils.

Niagara Group.* The upper part of this formation consists of red, yellow, and ash-colored shales, with compact limestones, variegated with bands and nodules of chert.

Halysites catenularia, *Columnaria inequalis*, *Calymene Blumenbachii*, and *Caryocrinus ornatus*, are the most characteristic fossils.

Cape Girardeau Limestone. I am also indebted to Dr. Shumard for a description of this formation.

"It is, according to him, a compact, bluish-gray, brittle limestone, with a smooth fracture, in layers from two to six inches in thickness, with thin argillaceous partings.

"These strata contain a great many fossils, principally Trilobites and Crinoids. In a small slab, not more than three by three inches, I have counted four genera of Trilobites, namely, *Cyphaspis Girardeauensis*, *Acidaspis Halli*, *Proteus depressus*, *Asaphus*, *Nov. sp.* None of the Trilobites have been before mentioned in this country, and, so far

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* I am indebted to Dr. Shumard for the information possessed respecting the Niagara and *Lower Helderberg Groups*, and the *Oriskany Sandstone*, as I have not examined those formations.

as I can ascertain, the species are distinct from European forms. According to Barande, the first three genera occur in the greatest number in the Upper Silurian period, and are very sparingly represented in the Lower Silurian groups. The Crinoids belong mostly to the genera Glyptocrinus, Homocrinus, and Tentaculites, and Palaeaster; and the shells to Orthis, Leptaena, and Turbo, — all being of undescribed species."

"These strata occur on the Mississippi river, about one mile and a half above Cape Girardeau. Thickness, forty to fifty feet."

LOWER SILURIAN.

We have thus far observed ten formations belonging to this series.

Hudson River Group	220 feet.
Trenton Limestone	360 "
Black River, and Birds-eye Limestone	75 "
1st Magnesian Limestone	200 "
Saccharoidal Sandstone	125 "
2d Magnesian Limestone	230 "
2d Sandstone	115 "
3d Magnesian Limestone	350 "
3d Sandstone	60 "
4th Magnesian Limestone	300 "

Hudson River Group.

There are three formations, which we have referred to this group.

1st. Immediately below the Oolite, of the Onondaga limestone, in the bluffs both above and below Louisiana, we find some forty feet of blue, gray and brown, argillaceous, magnesian limestone. The upper part of these shales is in thick beds, presenting a dull conchoidal fracture, and containing *Asaphus megistos*, and *Calymene senaria*.

The lower part of this division becomes more argillaceous, and has several thin beds of bluish-gray, crystalline limestone, intercalated, which contain many fossils of the following species: — *Leptaena sericea*, *L. alternata*, *L. planumbona*, *Orthis jugosa*, *O. subquadrata*, and *Rhynconella capax*.

There are, also, strata of calcareo-arenaceous slate, in the same position, filled with remains, which I am unable to distinguish from Prof.

Hall's *Palaeophycus virgatus*, and another contorted species, smaller than No. 2, pl. 70, of Prof. Hall's Report. There are, also, beds of slate, similar to those above mentioned, at the base of these shales, whose surfaces are covered with great numbers of the *Lingula ancyloidea.*

2d. On the Grassy, three and a half miles north-west of Louisiana, about sixty feet of blue and purple shales are exposed, below the beds above described. They contain three species of *Lingula*: *Lingula quadrata*, *L. fragilis*, and still another, not named.

The first resembles the *L. quadrata* of Hall, but is destitute of the "radiating striæ" of that species, and is larger; it is more like the variety from the Trenton limestone, than that from the Hudson river group.

3d. Under the 2d division are some twenty feet of argillo-magnesian limestone, similar to that in the 1st division, interstratified with blue shales. *Orthis subquadrata*, *O. jugosa*, *Leptaena alternata*, *Rhynconella capax*, and *Asaphus megistos*, are abundant.

These rocks crop out in Ralls, Pike, and Cape Girardeau counties. On the Grassy, a thickness of 120 feet is exposed; and they extend below the surface to an unknown depth.

Trenton Limestone.

The upper part of this formation is made up of thick beds of hard, compact, bluish gray and drab limestone, variegated with irregular cavities, filled with greenish materials; while the beds below are filled with irregular cylindrical portions, which readily decompose on exposure, and leave the rock perforated with numerous irregular passages, that somewhat resemble those made in timber by the *Teredo navalis.* The appearance of the rock, when thus decomposed, is very singular, and is a well-marked character of this part of the formation. The decomposed, honey-combed portions are most admirably adapted to ornamental rockwork, in gardens and yards. These beds are exposed on the plank road, from Hannibal to New London, north of Salt river, and are seventy-five feet thick. Below them are thick strata of impure, coarse, gray and buff, crystalline, magnesian limestone, with many brown earthy portions, which rapidly disintegrate on exposure to atmospheric influences. This part may be seen in the bluff of Salt river, near the plank road, 150 feet thick. The lower part is made up of hard blue and bluish gray, semi-compact silico-magnesian limestone, interstratified with light buff and drab, soft and earthy magnesian beds. Fifty feet of these strata crop out at the quarries south of the plank road bridge over Salt river, and on Spencer's creek, in Ralls county. The middle beds sometimes pass into a pure white crystalline marble of great beauty, as at Cape Girardeau.

Organic Remains. Fossils are abundant in all parts of the formation. *Leptaena deltoidea, L. sericea, L. alternata, Orthis pectinella, O. testudinaria, O. tricenaria, Rynconella capax, Murchisonia gracilis, M. bellicincta, Receptaculites sulcata,* and *Chaetetes lycoperdon,* are most common.

BLACK RIVER AND BIRDS-EYE LIMESTONE.

"They are bluish gray or dove-colored, compact, brittle limestones, with a smooth conchoidal fracture. The beds vary in thickness from a few inches to several feet." "Near the base, the rock is frequently traversed in all directions by vermicular cavities and cells."

Gonioceras anceps, Ormoceras tenuifolium, Cythere sublevis, are the most abundant fossils.

1ST MAGNESIAN LIMESTONE.

This formation is developed in many parts of the State. It is usually a gray or buff, crystalline, cherty, silico-magnesian limestone, filled with small, irregular masses of a soft white or greenish yellow, silicious substance, which rapidly decomposes when exposed, and leaves the rock full of irregular cavities, and covered with rough, projecting points. These rugged, weather-worn strata crop out in the prairies, and cap the picturesque bluffs of the Osage in Benton, and the neighboring counties.

These beds often pass into a homogeneous buff or gray crystalline magnesian limestone, which is frequently clouded with blue or pink, and would make a good fire-rock and building stone. At other places, the strata become compact, hard, and clouded, as above, forming a still more beautiful and durable marble.

Some of the upper beds are silicious, presenting a porous, semitransparent, vitreous mass, in which are disseminated numerous small, globular, white, enamelled oolitic particles. They are sometimes in regular and continuous strata, at others in irregular mass, presenting mammillated and botryoidal and drusy forms of this beautiful mineral. In some parts of Benton and the neighboring counties, these masses, left by the denuded strata, literally cover the surface, and render the soil almost valueless for ordinary cultivation. Other strata abound in concretions, or organic forms, which resemble wooden button-moulds, with a central aperture and one convex surface. Masses of calcareous spar are quite abundant in the upper beds. But the lower part of this formation is made up of thin, regular strata, of a soft earthy, light drab or cream-colored silico-argillaceous magnesian limestone.*

Above the beds already described, we find, in several places in the State, a succession of hard, silicious, dark bluish gray semi-crystalline limestones, interstratified with grayish-drab earthy magnesian varieties, all in regular layers destitute of chert. These strata have been joined to the 1st Magnesian limestone, with the expectation that they may prove distinct from the Calciferous sandrock and the 1st Magnesian limestone, and be identified with the Chazy limestone, or some other formation. *Straparollus laevata*, a small variety of *Cythere sublevis*, and a large *Orthoceras*, have been observed.

SACCHAROIDAL SANDSTONE.

This formation is usually a bed of white friable sandstone, slightly tinged with red and brown, which is made up of globular concretions and angular fragments of limpid quartz. It presents very imperfect strata, but somewhat more distinct lines of deposition, variously inclined to the planes of stratification.

This interesting formation has a wide range over the State. I have seen it in Ralls, Boone, Saline, Cooper, Moniteau, Pettis, Benton, Morgan, Hickory, St. Clair, Cedar, Polk, and Dallas; and Drs. Shumard and Litton observed it in Perry, St. Francois, Franklin, Ste. Genevieve, and other counties.

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* This variety of Magnesian limestone is generally called "Cotton Rock" in many parts of the State.

Its thickness is very variable, from one to 125 feet. At times it thickens very rapidly, so much so, as to increase thirty or forty feet in a few hundred yards. In a bluff about two miles north-west of Warsaw, is a very striking illustration of this change of thickness. This sandstone crops out along the bluff, between the 1st and 2d Magnesian limestone, and in a few yards decreases in thickness from twenty feet to one. Where thinnest, it is semi-vitreous, and the line of demarcation between it and the limestones is very distinct.

Near the same place is a locality where the sandstone thickens so rapidly as to present the appearance of a dyke, cutting off the strata of limestone above and below that formation. I have had specimens broken from the junction of this dyke-like mass with the wall of the adjacent limestone, which are half sandstone and half limestone, showing the two rocks firmly cemented together. On Bear creek, near Warsaw, as shown in the Second Annual Report of the Missouri Survey, at Hermann, and in many other places, are very striking instances of the dyke-like development of this rock; but I must admit that such a freak among sedimentary rocks I have never observed in any other formation. One might give a satisfactory reason for its penetrating the strata above; but by what process of nature it was made to cut off the beds below, is not so obvious. There is, perhaps, a possibility that, after the deposition of the 2d Magnesian limestone, the waters, which deposited the silicious matter of the sandstone, first cut a channel in the upper strata of the limestone. But future investigations may enable us to solve the difficulty more satisfactorily.

A very large *Orthoceras* is found in this sandstone.

2D MAGNESIAN LIMESTONE.

The lithological characters of this formation are very much like those of the 1st Magnesian limestone, above described. The following section from the bluffs of the Osage, above Warsaw, will give an idea of its general character: —

No. 1. 12 feet. 1st Magnesian limestone.
No. 2. 4 feet. Saccharoidal sandstone.
No. 3. 15 feet of soft, earthy, fine-grained, yellowish-white or drab

silico-magnesian limestone, with a conchoidal earthy fracture, in beds from half of an inch to one foot thick, interstratified with thin layers of bluish silico-argillaceous magnesian limestone. It is called "*Cotton Rock.*"

No. 4. 1 foot of coarse-grained crystalline greenish brown limestone, in thin laminæ. The crystals are as large as buck-shot, and give the rock a brecciated appearance.

No. 5. 8 feet of limestone, like No. 3, interstratified with chert.

No. 6. 10 feet of compact buff silicious limestone, filled with heavy spar and iron pyrites, some parts so variegated with flesh-colored spots as to present the appearance of a breccia — a beautiful and durable marble.

No. 7. 3 feet, coarse gray brown and buff crystalline magnesian limestone, filled with masses and veins of calcareous spar.

No. 8. 1 foot, like No. 3.

No. 9. 5 feet, like No. 7.

No. 10. 5 feet of hard, compact, gray silicious limestone, interstratified with chert and "Cotton Rock."

No. 11. 1 foot of yellowish-gray Saccharoidal sandstone.

No. 12. 4 feet, like No. 10.

No. 13. 10 feet, like No. 3.

No. 14. 5 feet, semi-oolitic sub-crystalline hard gray silicious limestone, interstratified with compact flesh-colored silicious beds.

No. 15. 6 feet of soft buff fine-grained magnesian limestone, interstratified with compact flesh-colored silicious limestone.

No. 16. 25 feet of coarse gray and buff silico-magnesian limestone, variegated by cavities filled with a white or yellowish pulverulent silicious substance, which decomposes on exposure, and leaves the rock porous. It is an excellent fire-rock.

No. 17. 4 feet, like No. 14.

No. 18. 10 feet, like No. 15. Strata undulating.

No. 19. 2 feet of fine, compact, flesh-colored silicious limestone.

No. 20. 8 feet of hard, gray crystalline semi-vitreous calcareous sandstone, with chert interspersed.

No. 21. 20 feet slope to water.

2D SANDSTONE.

This is usually a brown or yellowish-brown fine-grained sandstone, distinctly stratified in regular beds, varying from two to eighteen inches in thickness. The surfaces are often ripple-marked and micaceous. It is sometimes quite friable, though generally sufficiently indurated for building purposes. The upper part is often made up of thin strata of light, soft and porous, semi-pulverulent sandy chert or hornstone, whose cavities are usually lined with limpid crystals of quartz. Fragments of these strata are very abundant in the soil and on the ridges, where this sandstone forms the surface of the rock. It sometimes becomes a pure white fine-grained soft sandstone, as on Cedar creek, in Washington county, in Franklin, and other localities.

3D MAGNESIAN LIMESTONE.

This limestone is exposed in the high and picturesque bluffs of the Niangua, in the neighborhood of Bryce's spring, where the following strata were observed:—

No 1. 50 feet of the 2d Sandstone.

No. 2. 80 feet of gray and crystalline silico-magnesian limestone, somewhat clouded with flesh-colored spots and bluish bands. It is regularly stratified in thick beds, some of which have many cells filled with a white pulverulent silicious substance, while others are ferruginous and semi-oolitic. It contains very little chert.

No. 3. 50 feet of blue and white ferruginous chert, interstratified with hard, compact, and flesh-colored silicious limestone.

No. 4. 190 feet, like No. 2, save some beds are hard, compact, buff, or flesh-colored silicious limestone.

No. 5. 20 feet of light-drab fine-grained crystalline silico-magnesian limestone, often slightly tinged with peach-blossom, and beautifully clouded with darker spots and bands of the same hue or flesh color. It is distinctly stratified in beds of medium thickness.

No. 6. 50 feet, like No. 2.

No. 7. 30 feet of the *3d Sandstone* crops out lower down.

3D SANDSTONE.

This is a white Saccharoidal sandstone, made up of slightly cohering, transparent, globular, and angular particles of silex. It shows but little appearance of stratification, yet the well-marked lines of deposition, like those of a Missouri sand-bar, indicate its formation in moving water.

4TH MAGNESIAN LIMESTONE.

This presents more permanent and uniform lithological characters than any of the other Magnesian limestone. It is usually a grayish-buff, coarse-grained, crystalline magnesian limestone, containing a few cavities filled with less indurated silicious matter. Its thick uniform beds contain but little chert. The best exposures of this formation are on the Neangua and Osage rivers.

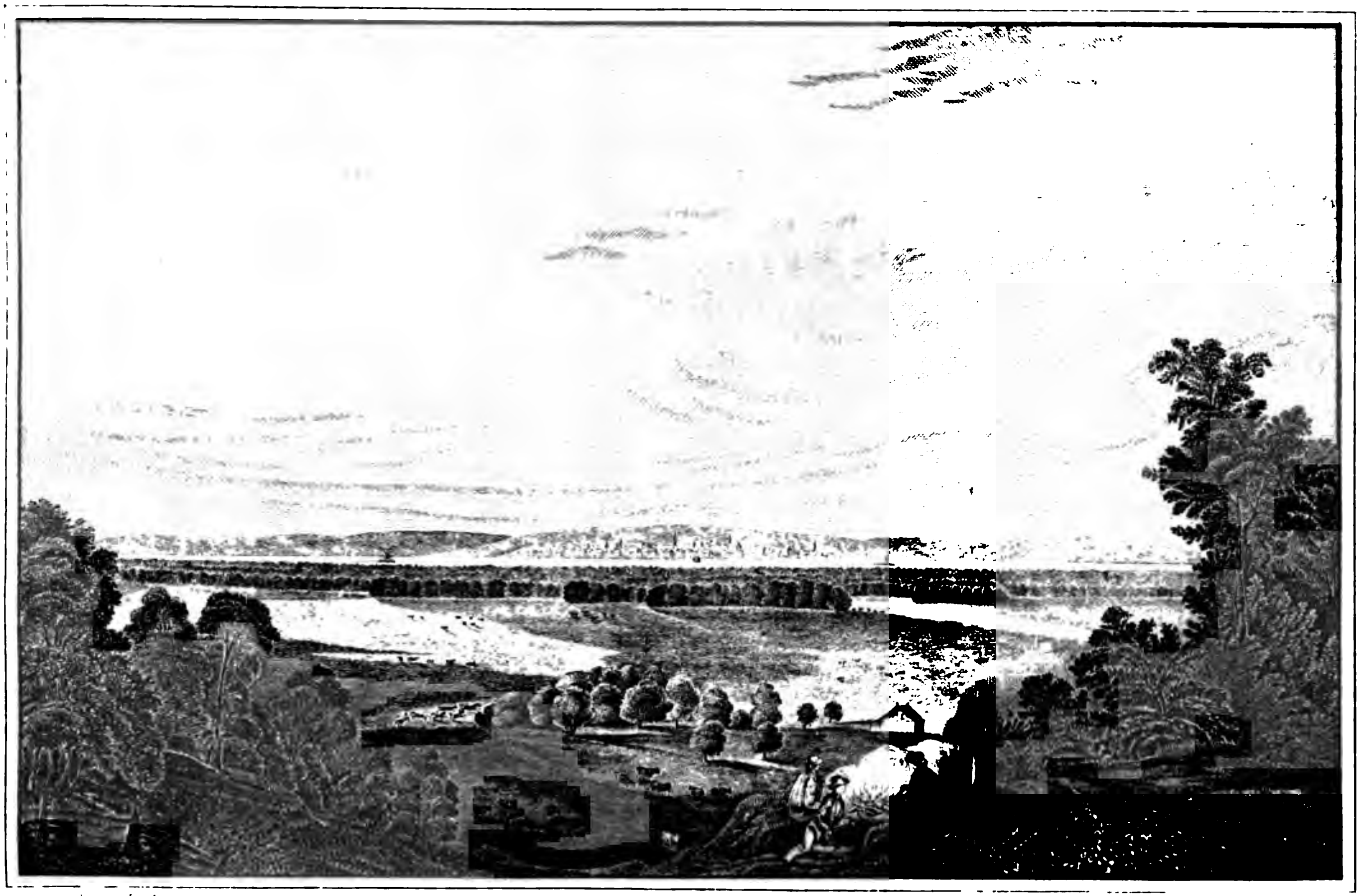
This *Magnesian Limestone Series* is very interesting, both in its scientific and economical relations. It covers a large portion of southern and south-eastern Missouri, is remarkable for its extensive caves and springs, and contains all the vast deposits of *lead*, *zinc*, *copper*, *cobalt*, *haematite*, *ores of iron*, and nearly all the marble beds of the State. They indeed contain a large part of all our mineral wealth.

The lower part of the 1st Magnesian limestone, the Saccharoidal sandstone, the 2d Magnesian limestone, the 2d Sandstone, and the upper part of the 3d Magnesian limestone belong, without doubt, to the age of the Calciferous sand-rock; but the remainder of the series may prove to be Potsdam sandstone.

Igneous Rocks.

There are a series of rounded knobs and hills in St. Francois, Iron, Dent, and the neighboring counties, which are principally made up of *granite*, *porphyry*, and *greenstone*. These igneous rocks contain those wonderful beds of *Specular Iron*, of which *Iron Mountain* and *Pilot Knob* are samples.

These mountains of iron and igneous rocks are older than the oldest of the stratified rocks above described; as the beds of the latter rest against the sides of the former, without exhibiting signs of any considerable disturbance.



View of Quincy and the Mississippi bottom, NOTE. I am indebted to Maj. Hawn, of the Linear Survey of Kansas, for all the geological facts on the accompanying map of Kansas, save what was derived from my own observations along the eastern border. The scientific world will be greatly indebted to Maj. Hawn for his diligence in collecting and recording geological data, while engaged in his survey.

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2. QUATERNARY DEPOSITS OF MISSOURI. By Prof. G. C. SWALLOW, State Geologist of Missouri.

PREVIOUS to the commencement of the Geological Survey of Missouri, but little effort had been made to trace out and classify the various deposits of the Quaternary system of the Mississippi valley. This fact, and the vast importance of these formations, both in our scientific and economical geology, have led us to undertake a careful investigation of this system, as developed in our State. The results of our early investigations were given in the second Annual Report of the Missouri Survey, in 1854. This paper will include the facts there recorded, and those observed in our subsequent examinations, that it may present a full view of the present state of our knowledge upon this subject.

When it is remembered that these formations contain the entire geological record of all the cycles from the end of the Tertiary period to the present time, and that their economical value is greater than that of all the other formations combined, I shall need no apology for entering somewhat into details in recording the phenomena they present.

The *Quaternary System* comprises the drift and all the deposits above it — all the strata included in the Alluvium and Diluvium of former authors. There are, within this period, four distinct and well-marked formations in this State, which we have thus named in the order of their stratigraphical position: — *

ALLUVIUM.	BLUFF.
BOTTOM PRAIRIE.	DRIFT.

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* See the general section of the rocks of Missouri accompanying my paper on that subject.

All of the latest deposits — all that have been formed since the present order of things commenced upon our continent — are included in the

Alluvium.

All the deposits observed in the State, belonging to this formation, are: —

1st. *Soils.*
2d. *Pebbles and Sand.*
3d. *Clays.*
4th. *Vegetable Mould, or Humus.*
5th. *Bog Iron Ore.*
6th. *Calcareous Tufa.*
7th. *Stalactites and Stalagmites.*

1st. *Soils* are a well-known mixture of various comminuted and decomposed mineral substances, combined and mingled with decayed vegetable and animal remains, all comprising those ingredients peculiarly adapted to the nourishment of the vegetable kingdom. They are formed by the action of water, particularly in the form of rain and dews, cold, heat, and other atmospheric influences, together with the co-operation of the vegetable and animal kingdoms.

The process by which soils are formed is one of the most beautiful and wonderful in nature. By a careful examination of what is transpiring in this great laboratory of nature, we may easily detect that process. If a rock, fresh from the quarry, be exposed, its surface will soon present a dull, earthy appearance, which is caused by a disintegration of its surface by atmospheric influences. Fine particles have been separated from the mass, and this meagre coating of decomposing mineral matter will soon become the resting-place of numerous microscopic germs, which will be developed into a minute growth of lichens. These, in turn, will decay, and add their remains to the pulverized particles, and prepare them to sustain a more vigorous growth of herbs, and to become the abode of the small insects and worms, which will burrow in their recesses, feed upon the increasing vegetation, and swell the mass, both by their mechanical agency, and by adding their exuviæ to the accumulating soil. Larger plants and animals will accelerate the process by their more powerful agencies, and by the greater amount contributed by their decaying remains. Thus, by almost imperceptible increments, our rich, deep soils have been accumulated.

But the soils of Missouri are made up by the mingling of organic matter with the comminuted marls, clays, and sands of the Quaternary deposits, which cover all parts of the State with a vast abundance of the very best materials for their rapid formation. Hence the soils of the State are very deep and wonderfully productive, save in those limited localities where the materials of the Quaternary strata are unusually coarse, or entirely wanting. But I shall speak more particularly of the soils while treating of the formations on which they are formed.

2d. *Pebbles and Sand.* Many of our streams abound in water-worn pebbles, which constitute their beds, and form bars along their margins and across their channels. These pebbles were derived from the drift and the harder portions of the adjacent rocks. They vary in size according to the transporting power of the streams in which they are found.

The economical value of these pebbles for roads and streets, and the obstruction they often present to navigation, as in the Osage, give them unusual importance in our geology. The Osage, Gasconade, Niangua, Marais des Cygnes, Sac, and Spring rivers, of the south; and the Salt, South, North, Fabins, and Charitan, of the north, all furnish good and abundant examples of these deposits, which have been formed by the action of those streams.

Sand is the most abundant material in the alluvial bottoms of the great rivers in the State. Vast quantities of it are constantly borne along by the irresistible current of the Missouri. Its whirling, rolling, turbulent waters form of it extensive bars in incredibly short periods, which they again wear away, often still more rapidly than they were formed.

These sand-bars, so common in this stream, frequently extend along its bed several miles, with a breadth varying from one to five or six furlongs, and limited in thickness only by the depth of the water. A slight fall in the river leaves these vast sand-beds dry, when their surfaces are soon covered by a growth of weeds, interspersed with young willows and cotton-wood. The fickle stream, however, seldom leaves these sand-beds to a long repose, but returns to its old channel by a rapid removal of their loose materials.

A disaster of the ill-fated steamer, *Timour No.* 2, presents a good illustration of the rapidity with which the Missouri forms and destroys these extensive deposits of sand. In the fall of 1853, this steamer ran upon a sand-bar, and was soon left high and dry, some seventy-five or one hundred yards from the water, with a fair prospect of leaving her timbers to decay in the young forest of willows and cotton-wood, which would soon spring up around her.* But the current changed, and cut its way through the sandy stratum upon which the boat rested, and floated her away, uninjured, to the great city of the West. As these sand-bars are cut away, their perpendicular faces present beautiful illustrations of their stratification, which is usually very irregular and complicated, as might be expected from the changeable character of the current.

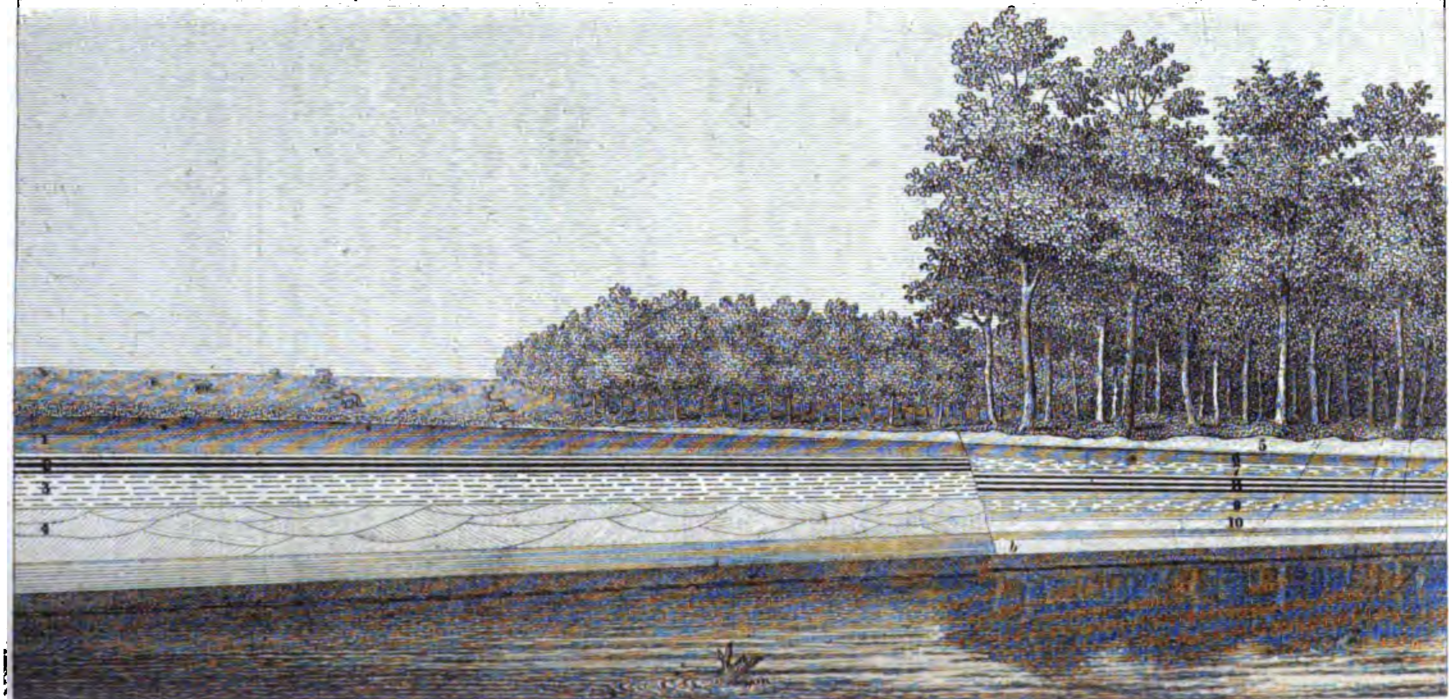
Fig. 2, sketched from a sand-bar in the Missouri river, two miles above Wayne City, presents a good example of their stratification. But water is not the only agent engaged in producing the irregular stratification of the sand-bars of the Missouri and Mississippi rivers. When these sand-bars become dry by exposure, the winds easily transport and rearrange their light and fine materials. Such quantities are moved by high winds, that the entire channels of the rivers are obscured by the dense clouds of moving sand. The stratification of the sand-beds thus formed is very interesting and complicated, and aids us in explaining some examples of stratification observed in the older rocks.

At high stages of water, both the Missouri and Mississippi overflow their low bottoms, and leave deposits of a grayish-brown, or a grayish-yellow sand, similar to that in the sand-bars mentioned above. The thickness of these beds depends upon the height and continuance of the overflowing waters, varying from a mere perceptible stratum to several feet.

That from the flood of 1844 is very conspicuous, throughout the length of the Missouri bottom. It is sometimes six or eight feet thick, particularly in low bottoms, so heavily timbered as to obstruct the current.

At the lower end of the Waconda prairie, this deposit is very evenly distributed over its surface; but it increases in thickness as the prairie descends to the lower timbered bottoms, lower down the stream, where

* The sand of the Missouri, usually grayish-brown and fine-grained, contains a considerable quantity of lime and clay and vegetable matter, which render it very productive.



Dr by R B Price On the Lithographic Limestone F.L. Lith by [illegible]

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it is six or seven feet, and its surface becomes very irregular, like the surface of a lake, when disturbed by a high wind, or a *chopped sea.*

Sect. 2, showing the lower extremity of Waconda prairie and the cotton-wood bottom below, finely illustrates these phenomena. Nos. 6, 7, 8, 9, and 10 represent older strata, and No. 5, that left by the flood of 1844. The small timber in the middle is a young growth of cotton-wood, which has sprung up subsequent to that event, and the larger trees just below are the older growth, which obstructed the waters flowing through the bottom, and thus caused the more abundant and irregular deposit there observed.

Similar phenomena are exhibited in the bottoms opposite St. Charles and Jefferson City, and at many other places on the Missouri, and several localities in New Madrid and Pemiscot counties, on the Mississippi.

These sands were doubtless derived from those extensive sand-stone formations on the Platte,* and other tributaries of the Missouri. It is nearly all silex, but contains enough calcareous and argillaceous matter to render it fertile, as is abundantly proved by the growth of weeds and willows, cotton-wood and sycamores, which immediately spring up on these sand-bars whenever they are exposed above the water. There are many points on the Missouri, as in the bottom opposite St. Charles, where a thrifty growth of young timber may be seen on the sand deposits of 1844.

3d. *Clays.* These are dark, bluish-gray, argillaceous strata, rendered more or less impure by fine silicious, calcareous, and decomposed organic matter. When the floods of the Mississippi and the Missouri subside, the lagoons, sloughs, and lakes are left full of turbid water. The coarser materials soon settle into a stratum of sand, but the finer particles more gradually subside, and form the silico-calcareous clays of their alluvial bottoms. Thus, after each flood, new strata of sand and clay are deposited, until the lakes and sloughs are silted up.

The thickness of each stratum of sand depends upon the height and continuation of the flood which forms it; but that of the clay-beds is governed more by the time between the overflows, and is very variable, ranging from the tenth of an inch to ten feet. The argillaceous

* The Platte is a rapid stream, and brings down large quantities of sand, though its waters are not so turbid as those of the Missouri, either above or below their junction.

materials which formed them were doubtless derived from the Cretaceous and Tertiary clays of the upper Missouri and its tributaries, whence, as from the *Mauvaises Terres*, such vast quantities of a similar material have been removed by denudation.

4th. *Vegetable Mould or Humus* is dark-brown or black deposit of decayed vegetable matter, containing variable, though small, quantities of fine silicious and argillaceous particles. When wet, it is very soft and plastic, and quite black; but when dry, it separates into angular cuboidal fragments, which readily crumble into a dark, very light, impalpable powder. In these beds of almost homogeneous humus, leaves and stems of trees are sometimes found in a tolerable state of preservation.* The process by which these strata of humus are deposited is very obvious. When the lakes and sloughs of these bottoms are so far filled up as to sustain vegetable life, the decay of the annual growth, and of the foreign matter which falls or floats into these waters, forms a stratum of humus at the bottom, over the beds of clay and sand, previously deposited by the floods and still waters. Another overflow gives another succession of sand and clay; and the succeeding annual crop of vegetable matter, another stratum of humus.

These changes have often continued until several series of these deposits were formed; but when the bottoms of those bodies of water had been thus raised so high above the river, that the floods less frequently flowed into them, the deposits of sand diminished, and the long, quiet intervals favored the deposition of clay and humus. In time, these shallow waters became mere marshes, where a rank vegetation rapidly formed thick beds of vegetable mould, for the support of the magnificent forests which now occupy the sites of those ancient lakes and sloughs.

Such is the process by which the succession of sands, clays, and humus, in those alluvial bottoms, has been deposited. Whence it is easy to see why the sands are most abundant at the bottom, when the waters from the river floods would more frequently overflow them; the clays in the middle, when the waters would be rarely disturbed by overflows; and the humus or vegetable mould at the top, when a rank vegetation prevailed and inundations were rare.

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* Do these preserved leaves and stems prove that these beds of humus were not derived from decayed vegetables?

Sections 3 and 4 of our second annual report are good illustrations of the manner in which these strata of sand, clay, and vegetable mould succeed each other in the alluvial bottoms of our two great rivers. Such is the structure of the vast alluvial plains bordering the Missouri and Mississippi rivers.

The bottom of the Missouri, extending from the Iowa line to its mouth, is about seven hundred miles long, and five broad, presenting an area of 3,500 square miles; more than one half of this, say 2,000 square miles, may be set down as alluvium, while the river, "bottom-prairies," and lakes, occupy the remainder.

On the Missouri side of the Mississippi bottom, there are about 4,300 square miles of alluvial bottom of a similar character. Thus the alluvial bottoms of our two great rivers alone, give more than 4,000,000 acres of land based upon these strata of sands, clays, marls, and humus. And beside, the quantity is constantly increasing by the silting up of the sloughs and lakes, as above described.

The soil formed upon these alluvial beds is deep, light, and rich, almost beyond comparison, as is abundantly proved by the immense burden of timber * growing upon it, and by the unparalleled crops of hemp, tobacco, and corn, harvested from its cultivated fields.

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* In the fall of 1856, our surveying party measured several trees in south-eastern Missouri. The following measurements were obtained from some of the largest trees of those species: —

In Stoddard Co., a Beech, *Fagus ferruginea*, 18 feet circumference, and 100 high.

In Stoddard Co., a Tupelo Gum, *Nyssa grandidentata*, 30 feet circumference, and 120 high.

In Dunklin Co., a Catalpa, *Catalpa bignonioides*, Gray, 10 feet circumference, and 90 high.

In Pemiscat Co., an Elm, *Ulmus Americana*, 22 feet circumference, and 100 high.

In New Madrid Co., a Cypress, *Taxodium distichum*, 29 feet circumference, and 125 high.

In Cape Girardeau Co., a Sweet Gum, *Liquidambar Styraciflua*, 15 feet circumference, and 130 high.

In Cape Girardeau Co., a White Ash, *Fraxinus Americana*, 18 feet circumference, and 110 high.

In Mississippi Co., a Spanish Oak, *Quercus falcata*, 28 feet circumference, and 100 high.

In Mississippi Co., a Sycamore, *Platanus occidentalis*, 43 feet in circumference. This sycamore was hollow, and the cavity measured 15½ feet in one diameter, and 13 feet in the other.

5th. *Bog-Iron Ore* is deposited from several chalybeate springs. Large quantities of the hydrated oxide have accumulated near a fine spring, some two miles west of Oseola, and a small amount from another near Sharpsburg, in Marion county. But the most important deposits of bog-iron are situated in the cypress-swamps, and other low wet portions of south-eastern Missouri. Extensive beds were traced out in lake St. John, extending from the "Stark Glades," in Scott county, to the "Iron-Ore Ford," in N. Madrid. Several large beds were observed in the south-western part of Dunklin county, and two in the western part of Mississippi. These beds vary in thickness from one or two inches to two feet, while some of them are several miles in length. The quality of the ore is good, and the quantity very great,—almost fabulous.

6th. *Calcareous Tufa* has been found in several places in the State. In a ravine south of Parkville is a mass in which many specimens of moss are well preserved; and another similar deposit was observed under the bluffs near Bryce's spring, on the Niangua.

7th. *Stalactites and Stalagmites* are abundant in some parts of the State. Many beautiful specimens were observed in the extensive caves of Boone, Camden, and Greene counties.

Such are the alluvial deposits, so far as observed, in Missouri. Future investigations may bring to light others belonging to this formation.

Range and Thickness. Our alluvium is, as a matter of course, diffused throughout the entire State, as it comprises all the soils and other deposits now forming. It is, however, much more abundant in the valleys of our great streams. The thickness is often thirty or forty feet, though usually much less.

Bottom Prairie.

This important formation, in many respects, resembles that of the *alluvial bottoms* above described, with which it has usually been confounded by geologists; though agriculturalists have made a distinction.

There are, however, important differences:—

1st. The stratification in the prairie is much more uniform, and more regularly extended over wide areas.

2d. In the prairie formation, the strata are not so distinct, nor are they so purely silicious or argillaceous.

3d. It was evidently formed by agencies operating over the entire bottoms, whose action was more uniform and quiet, and continued uninterrupted through longer periods, than those now forming the alluvial deposits in the same bottoms.

4th. Where these two formations meet, one can usually trace out the line of demarcation. Either the strata of the prairie pass under those of the alluvium, or are cut off and replaced by them. Instances of both these changes may be observed at the lower end of Waconda prairie, as shown in sect. 2. The upper stratum of the prairie, No. 1, passes under No. 5, of the alluvial bottom, and continues in that position several hundred yards; while Nos. 1, 2, 3, and 4 of the former are cut off at *b*, and are replaced by Nos. 6, 7, 8, 9, and 10 of the latter.

5th. The alluvial bottom is continually increased at the expense of the prairie, through the action of the rivers. The current is constantly cutting away the prairie, forming new channels, and filling up the old ones with drift and silt. This explains the fact that the strata of the prairie are frequently cut off, and others quite different set in, as we pass from it to the timbered bottoms, as illustrated in sect. 2. The part of that section under the large timber was once the channel of the river, and has been filled up by the process explained above, in describing the last formation. At high stages of water the lower portions of the prairie are overflowed, and deposits of sand are left on its surface, which are soon covered with willows, sycamores, or cottonwood, as is shown by the growth which has sprung up on the deposits from the flood of 1844.

6th. No causes now in operation could, at the present level of the country, produce a formation of such extent and uniform structure as the bottom prairies.*

Such are some of the facts which have convinced me that this is an older formation, and one entirely distinct from the alluvial bottoms. Several facts show it to be distinct from, and newer than, the bluffs or

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* Some of the bottom prairies of the Missouri are, at least, thirty miles long, and from ten to twenty feet broad, as the Huppan-Kuty of Nicolet, above the mouth of the Sioux river, and the Waconda, in Carrol county. And these are probably only fragments of one which was once continuous from the former to the mouth of the Missouri.

loess. Its composition, structure, and position are entirely different, and in many places the bottom prairie rests non-conformably upon the bluff, as at St. Joseph, and the mouth of the Big Nemaha.

This formation, like the last, is made up of sands, clays, vegetable mould, variously interstratified.

The sand in the upper part is fine and yellowish-brown, like that of the Missouri sand-bars; but the lower beds are more purely silicious.

The clays are usually dark, bluish-brown, and marly, with more or less sand and humus intermingled.

The humus or vegetable mould has a brownish or black color; when wet, it is somewhat plastic, and slightly tenacious; when dry, it is brittle, and breaks into angular fragments, and can be easily reduced to an impalpable powder. These beds of humus were evidently formed by the growth and decay of plants in the localities where they are found.

Range and Thickness. This formation is confined to the bottoms of the Missouri and Mississippi rivers, and is more abundant and better characterized on the former. The bottom prairie is about half as extensive as the alluvial bottoms above described, and sustains a soil of equal fertility. This estimate will give us about 2,000,000 acres of these vastly rich savannas, all prepared by nature for the plough. Their agricultural capacities are scarcely inferior to any lands in the world, as is abundantly demonstrated by the mineral contents of the strata, and the products of the numerous flourishing farms located upon them.

The Organic Remains of the bottom prairie, are numerous and well preserved. All the shells of the bluff, save the *Helicina occulta*, have been found in it; but no remains of the elephant or mastodon have yet been detected. We have collected many species of trees, shrubs, and vines from these beds.

The character and position of the strata forming the bottom prairie show most conclusively, that the level of the country where they were deposited was somewhat different from its present condition, that bodies of almost still water covered the present valleys of our great rivers, and that the formation was coextensive with these river bottoms; and that subsequently such a change of level occurred, as gave the present rapid current to the waters passing through these valleys. The rapid waters cut channels in their soft beds, and left broad level areas dry, and subject to vegetable life. These "Bottom Prairies" extended from the mouth of the Sioux to the Mississippi, and probably from the St. Peter's to the Arkansas. Since that period, the rivers have been ever busy wearing away the bottom prairie and depositing the alluvial bottoms above described, until we have but few remnants, such as the Waconda and Hupan-Kuty, of the vast bottom prairie which once occupied these great valleys.

These beautiful savannas are almost universally called "*Bottom Prairie*," and I have proposed that as the geological name of the interesting formation on which they rest. The scenery of the alluvial bottom and the bottom prairie is well represented in Sections 2 and Plate 12.

BLUFF.

This formation rests upon the drift, as is obvious whenever the two formations are well developed. In many places, as at St. Joseph and at the mouth of the Big Nemeha, it is seen dipping beneath the beds of the bottom prairie. The bluff formation rests upon the highest ridges and river bluffs, and descends along their slopes to the lowest valleys. The bottom prairie is confined to the river bottoms, and was deposited in horizontal beds between the bluffs. Thus, while the bottom prairie occupies a higher geological horizon, the bluff is usually several hundred feet above it in the topographical.

This formation, when well developed, usually presents a fine, pulverulent, obsoletely stratified mass of light grayish buff, silicious and slightly indurated marl. Its color is usually variegated with deeper brown stains of oxide of iron. The bluff above St. Joseph exhibits an exposure of it 140 feet thick, presenting its usual characteristic features. When but sparingly developed, it generally becomes more argillaceous, and assumes a deeper brown or red color, as on the railroad south of Palmyra, where it is a dark brick-red, tinged with purple. In some places, the ferruginous and calcareous matter increases, and we find concretions of marl and iron-stone, either disseminated through or arranged in horizontal belts. At other places, it has more arenaceous matter, and is much more decidedly stratified, as at a point one mile above Wellington, and in the bluff at St. Joseph.

These are the only places seen where the stratification assumed the irregular appearance so often presented by sand-bars. It is barely possible that this stratified sand is a portion of *altered drift;* but the beds between it and the drift, having the usual appearance of the bluff, militate against such a supposition.

The bluff formation is often penetrated by numerous tubes or cylinders, about the size or thickness of pipe stems, some larger and others smaller. They are composed of clay, carbonate of lime, and oxide of iron, being argillo-calcareous oxide of iron, or calcareous clay-ironstone. But it is not so easy to say how they were formed. Several facts may aid us in determining this matter. These tubes penetrate the formation in all directions, and are most abundant near the surface, though some extend to the depth of twenty feet. The space for some half inch around each tube, more or less according to its size, is of a much lighter color, as if the coloring matter (oxide of iron) had been extracted.

The same appearances were observed around the green and dry roots of the white oak (Quercus alba), which had penetrated the same formation. Qualitative analysis proved these same roots to contain a large portion of oxide of iron. And besides, oak-wood always contains a large portion of that metal and manganese. An analysis of its ashes by Saussure gave 2.25 per cent. of the oxides of these metals, while the analysis of "oak-wood mould," or the decayed wood, by the same chemist, gave 14 per cent. of the same oxides.

It is thus made manifest that oak-wood contains iron, which must have been absorbed through the roots from the earth. This fact readily explains the loss of the iron from the marl around the roots, and around the tubes, provided they were once oak-roots. But the question naturally arises, how these roots became tubular. They were seen in the various stages of decay, and the woolly fibres of some had disappeared and left the bark in the form of a tube, still retaining its organic structure, though strongly impregnated with the oxide of iron and aluminum and carbonate of lime.

It may also be objected, that the roots of the oak do not penetrate to such depths; but, in the language of a poet and botanist,

> "Aesculus in primis; quae quantum vertice ad auras,
> Aetheras, tantum radice in Tartara tendit."

These facts have led to the conclusion, that these tubes of calcareous clay-ironstone are decayed roots of oak or other plants. In some localities, small holes, also, without any tubes of different material, penetrate this formation in great numbers, and are probably caused by similar agencies.

These phenomena have been thus minutely investigated, not merely as interesting scientific facts, but, also, as one of the most useful agricultural features of this preëminently valuable formation; for upon it, and sustained by its absolutely inexhaustible fertilizing resources, rest the very best farms of the Mississippi and Missouri valleys. These tubes and holes, also, constitute the *most thorough system of drainage* imaginable.

Range and Thickness. So far as my own observations extend, this formation caps all the bluffs of the Missouri, from Council Bluffs to its mouth, and those of the Mississippi, from the mouth of the Des Moines to that of the Ohio, and forms the upper stratum beneath the soil of all the highlands, both timber and prairie, of all the counties north of the Osage and Missouri, and also St. Louis, and the other Mississippi counties on the south.

According to Mr. Meek, its western or north-western limit is probably a few miles below Fort Pierre; Lyell traces a similar formation up the Ohio and further down the Mississippi; Dr. Owen mentions its existence on the Wabash river; and Dr. G. G. Shumard saw similar deposits on Red river.* The identity of the deposits at Council Bluffs, St. Joseph, Lexington, Booneville, Hannibal, St. Louis, and at Cape Girardeau, is placed beyond all doubt by the following facts:—

1st. They occupy the same geological position.

2d. They have the same topographical position on the tops of the bluffs.

3d. They present the same lithological and chemical characters.

4th. Nearly all the fossils are found at all those places, save, perhaps, the last.

5th. These localities are connected by an unbroken continuity of the same deposit.

Its greatest development in this State is in the counties on the Missouri, from the Iowa line to Boonville; but thence to St. Louis, it is not so thick. In some places, it is two hundred feet thick. At St. Joseph, it is one hundred and forty; at Boonville, one hundred; and

* Capt. Marcy's Report on Red river of Louisiana, p. 182.

at St. Louis, in St. George's quarry, and the Big Mound, it is about fifty feet; while its greatest thickness observed in Marion county was only thirty.

Organic Remains. The fossils of the Bluff are very numerous and interesting. Those which I have had time to determine are in the following catalogue: —

CATALOGUE OF BLUFF FOSSILS.*

No. 1.	*Cyclas*, Species undt.	Near mouth of Wolf river.
No. 2.	*Amnicola lapidaria*, Say,	Near St. Louis.
No. 3.	*Helix rufa*, De Kay,	St. Joseph Landing.
No. 4.	*Helix albolabris*, Say,	Half mile below Great Nemaha.
No. 5.	*Helix alternata*, Say,	Bellevue, Bluff City Landing, mouth Wolf river, and Lexington.
No. 6.	*Helix concava*, Say,	Bluff City Landing, and near St. Louis.
No. 7.	*Helix thyroidus*, Say,	Bluff City Landing.
No. 8.	*Helix profunda*, Say,	Lexington, mouth Little Nemaha, Bluff City Landing, etc.
No. 9.	*Helix multilineata*, Say,	Near mouth Big Nemaha, Little Nemaha, and Platte river.
No. 10.	*Helix clausa?* Say,	
No. 11.	*Helix striatella*, Anthony,	St. Louis, Boonville, below mouth Platte river and Big Nemaha.
No. 12.	*Helix monodon*, Rackett,	St. Louis.
No. 13.	*Helix electrina*, Gould,	St. Louis and Boonville.
No. 14.	*Helix arborea*, Say,	St. Louis.
No. 15.	*Helix indentata*, Say,	Below mouth Platte river.
No. 16.	*Helix hirsuta*, Say,	Bluff near St. Louis.
No. 17.	*Helix lineata*, Say,	Bluff City Landing, below mouth Platte, near mouth Big Nemaha.
No. 18.	*Helix minuta*, Say,	Bluff City Landing, below mouth Platte, Boonville, etc.
No. 19.	*Helix labyrinthica*, Say,	St. Louis.
No. 20.	*Helicina occulata*, Say,	Boonville and near St. Louis.
No. 21.	*Limnea fragilis*, Lin,	Bluff City Landing, below mouth Platte, Bellevue and Lexington.
No. 22.	*Limnea reflexa*, Say,	Bluff City Landing.
No. 23.	*Limnea umbrosa?* Say,	Near mouth Great Nemaha.
No. 24.	*Limnea* — 5 or 6 sp.,	Bluff City Landing, mouth Wolf river, below mouth Platte, etc.

* I am indebted to Mr. Meek for the arrangement of this catalogue.

No. 25.	*Physa plicata*, De Kay,	Bluff City Landing, near mouth Wolf river.
No. 26.	*Physa heterostropha*, Say,	Below mouth Platte and mouth Little Nemaha.
No. 27.	*Physa elongata*, Say,	Below mouth of Platte river.
No. 28.	*Physa gyrina*, Say,	Bluff City Landing and below mouth Platte.
No. 29.	*Physa* — Several undt. sp.,	Most of the above localities.
No. 30.	*Planorbis trivolvis*, Say,	Bluff City Landing, below mouth Platte, and at Big Nemaha, etc.
No. 31.	*Planorbis trivolvis*, (var.,)	Same as above.
No. 32.	*Planorbis armigerus*, Say,	Mouth Wolf river, and below mouth Platte.
No. 33.	*Planorbis* (undet.),	Below mouth of Platte river.
No. 34.	*Pupa armifera*, Say,	Bluff near St. Louis, Bluff City Landing, mouth Platte, etc.
No. 35.	*Pupa* (undet.),	Near St. Louis.
No. 36.	*Succinea obliqua*, Say,	Bluff City Landing, and below mouth of Platte river.
No. 37.	*Succinea campestris*, Say,	Bluff City Landing.
No. 38.	*Succinea ovalis?* Say,	Below mouth of Platte, Bluff City Landing, etc.
No. 39.	*Succinea* — 3 or 4 undet. sp.,	Same localities as above.
No. 40.	*Valvata tricarinata*, Say,	Bluff City Landing.

PLANTS.

No. 41.	*Seeds of Lithospermum*,	Nine miles below Bethlehem.

MAMMALIA.

No. 42.	*Castor Fiber-Americana*,	Near mouth Big Nemaha.
No. 43.	*Elephas primigenius*,	Bonne Femme Creek, Boone county.
No. 44.	*Mastodon giganteus*,	St. Louis.
No. 45.	*Molar of Ruminant*,	Near mouth Big Nemaha.
No. 46.	*Incisors of small Rodent*,	Near mouth Big Nemaha and mouth Wolf river.

I have collected from it, of the Mammalia, two teeth of the *Elephas primigenius*, the jawbone of the *Castor fiber Americana*, the molar of a *Ruminant*, and the incisor of a *Rodent;* of the Mollusca, seventeen species of the genus *Helix*, eight *Limnea*, eight *Physa*, three *Pupa*, four *Planorbis*, six *Succinea*, and one each of the genera *Valvata*, *Amnicola*, *Helicina*, and *Cyclas*, besides some others not determined.

These *lacustrine*, *fluviatile*, *amphibious*, and *land* species, indicate a deposit formed in a fresh-water lake, surrounded by land and fed by rivers. These facts carry back the mind to a time when a large portion of this great valley was covered by a vast lake, into which, from the surrounding land, flowed various rivers and smaller streams. We see the waters peopled with numerous mollusks, the industrious beaver building his habitation, the nimble squirrel, the fleet deer, the sedate elephant and huge mastodon, lords of the soil. There must have been land to sustain the elephant and mastodon and helices; fresh water and land for the beaver, and fresh water for the cyclas and limnea.

Some have supposed this formation was deposited by the rivers when their waters were at a higher stage. If it was deposited by the rivers, their waters were high enough to cover nearly all this and a large part of the adjoining States and territories, and quiet enough to be the abode of linneas, and to be called a lake.

I have proposed the title *Bluff* formation for this deposit, as it forms a large portion of, and gives the peculiar characters to, the bluffs so conspicuous and unique in the scenery about Council Bluffs and other portions of the Missouri valley, and as it forms the tops of the bluffs wherever it is developed.

Loess, the name of a similar formation on the Rhine, has been given to this by some geologists. But this would imply that these two formations are identical, when they may or may not be, so far as any proof has been given. It is true they are both fresh-water deposits, both have recent shells of the same genera, and in lithological and chemical characters, they are somewhat similar. But there are other deposits whose fauna and lithological and chemical properties are quite as similar to the bluff, and some of them more so, and yet they are more recent.

There is just as much evidence of the identity of the loess and the bottom prairie, as there is of the loess and the bluff; and still we know the bluff was formed long before the bottom prairie, and under a very different condition of this part of the continent. It may, also, be stated, that there is just as much evidence of the identity of the bluff and bottom prairie, as of the bluff and loess, and yet the bluff and bottom prairie are not identical. The fossils of all three formations only prove them to be quaternary.

There is, indeed, but little probability that two such vast fresh-water lakes existed at the same time on the two continents, with the ocean rolling between.

But it would seem impossible to identify formations so recent on separate continents, whose recent faunas are so widely different; as the deposits on these continents, though contemporaneous, would of necessity present faunas very distinct. Hence, if we make fossils our only guide in identifying them, it will be impossible to distinguish deposits formed since the present genera of animals and plants came into existence, and we shall be compelled to omit all distinctions between formations of the recent period, and to make all of our recent deposits identical with each other, and with all belonging to the same system in Europe and Asia; and this would deprive us of distinctions recognized in scientific, and almost indispensable in economical, geology.

I have been thus minute in my examinations of the bluff, the bottom prairie, and the alluvial formations, both on account of their vast importance to our agricultural interests, and the comparatively little attention geologists have given to them. It is to this formation that the central Mississippi and southern Missouri valleys owe their preëminence in agriculture. The most desirable lands of Iowa, Missouri, western Illinois, Kansas, and Nebraska, all rest upon the fine silicious marls of the bluff formation. Where it is best developed, in western Missouri, the soil is inferior to none in the country.

The scenery presented by the bluff formation is at once unique and beautiful, and gives character to nearly all the best landscapes on the lower Missouri. Plates I. and II. of the Missouri Reports give characteristic views of the scenery where this formation is well developed.

Drift.

This formation lies directly beneath the bluff, and rests upon the various members of the Palæozoic series, as they successively come to the surface of that system. In this formation there appear to be three distinct deposits:—

1st. What might be called an *Altered Drift* frequently appears in the banks of the Missouri river, as at the mouth of the Kansas, and in the bottom prairie below Brunswick, and at Waconda prairie, sect. 2, No. 4. These strata of sand and pebbles appear to be the finer materials of the drift, removed and rearranged by aqueous agencies subsequent to the Drift period, and prior to the formation of the bluff. The pebbles are from all the varieties of rocks found in the true drift, but are comparatively small.

2d. The Boulder formation, as it was left distributed by those powerful and widely extended agencies, which formed that deposit of the northern hemisphere. It is a heterogeneous stratum of sand, gravel, and boulders, all water-worn fragments of the older rocks. The larger part are from the Igneous and Metamorphic rocks, in place at the north, and the remainder from the Palæozoic strata, upon which they rest. The Metamorphic and Igneous rocks must have come from the northern localities of those strata, the nearest of which, according to Dr. Owen's report, is on the St. Peter's river, about three hundred miles north of St. Joseph. But the Palaeozoic fragments are usually from localities near where they rest, as shown by the fossils they contain, and are as *completely rounded* as those from the more distant points.

The largest boulders observed in Missouri are five or six feet in diameter. They are usually granite and Metamorphic sandstone.

3d. *Boulder Clay.* In northern Missouri, the Boulder formation just described often rests upon a bed of bluish or brown sandy clay, through which pebbles of various sizes are disseminated in greater or less abundance. In some localities this deposit becomes a pure white pipe clay.

Range and Thickness. The *Altered Drift* has been observed more frequently in the north-western part of the State, and is often twenty-five or thirty feet thick. The Boulder formation abounds in all parts of the State north of the Missouri, and exists in small quantities as far south as the Osage and Meramec. Its thickness is very variable, from one to forty-five feet. Its development is greater, the boulders larger, and those of a foreign origin more numerous, towards the north.

Its thickness varies from one to fifty feet. The Boulder clay is also most abundant in the northern part of the State, and is, in some places, more than one hundred feet thick.

Organic Remains. I have seen no fossils in this deposit, save a few logs in the Altered drift of the Missouri. Some of these are still sound, and burn quite well when dry, as we have proved by building our camp fires with them on several occasions.

There are other deposits, particularly in the middle and southern parts of this State, which are not genuine drift; and yet they bear a greater resemblance to that than any other formation, and occupy precisely the same stratigraphical position.

Beneath the alluvium of the bottoms, we often find deposits of pebbles similar to the genuine or Altered drift of the Missouri, but all the materials come from the neighboring rocks, and appear like the beds of ancient streams.

On the high lands there are, in the same position, numerous beds of angular fragments of the adjacent rocks, somewhat worn, and indiscriminately commingled with sands and clays.

Whether these deposits were formed by the same agencies which produced the drift, or by a part of them only, or by other causes, has scarcely been determined.

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3. On Three Comparative Sections of the Coal-Measures in Kentucky, and in Eastern and Western Pennsylvania. By J. P. Lesley, of Philadelphia.

My object in presenting these sections is twofold. While we note at this meeting such remarkable advancing steps of our science as the identification of the sub-palæozoic systems in Europe and America, and the finding of Keysport Potsdam fossils as a link between the Lower Silurian of Boston bay and Wisconsin, we should place on record also as part of the successes of the last year, and a very eminent part, the identification of the coal-beds of Pennsylvania, Kentucky, and Ohio by my friend M. Leo Lesquereux, whose unpublished results I take upon myself the responsibility of alluding to, simply to record the fact, and to communicate to those who are working with me in the same field the happiness I have myself experienced in seeing the evident final success of a task which, from a merely lithological and structural point of view, seemed hitherto hopeless. In 1841, '42, and '43, when the mass of materials collected by the gentlemen geologists of the Pennsylvania Survey fell into my hands, by direction of Professor Rogers, the chief of the survey, to be mapped, and thrown into vertical and horizontal sections, I spent many weeks in discussing hundreds of fragmentary sections of the coal-measures, both anthracite and bituminous, many of which I had made or verified myself, to realize what I supposed would be a very simple and easy consequence. In vain, however, I adjusted and readjusted these sections to each other in every variety of grouping. Although I felt instinctively and with increasing certainty as I went on, that an identity of the bituminous and anthracite beds existed; that the Monongahela and Alleghany systems, imperfectly made out as they were, would certainly be carried over their eastern outcrops, some day, when corrected and perfected, into the anthracite basins, and westward through Ohio into Kentucky; still no such scheme could be stated on paper by any method of exhibition at my command. The intervals were so variable over such small areas, and the beds themselves varied so much in appearance, that every extensive exhibition of them in series of vertical sections which I made, became a mass of confusion to the eye. The attempt was abandoned, and the sections were engrossed, in Mr. Rogers' Final Manuscript Report, separately, on the pages of the text in which they were severally described. The difficulty lay in the want of a palæontological guide. This has been furnished by the distinguished fossil botanist of Columbus, whom I feel honored to know and name, who has succeeded, after some years of the closest field-work, in laying the foundation for a perfectly systematic identification of our coals over our largest carboniferous areas. I need only mention his positive identification of the Lingula Umbonata and Lepidostrobus bed B everywhere, the identification of the Pomeroy coal-bed on the Ohio river, with the Gate vein at Pottsville, on the Schuylkill river, and the identification of the highest Curlew and Airdrie coal-bed of western Kentucky, with the great Pittsburgh bed of Pennsylvania, as typical examples.

My second object is to correct an important error in my Manual on Coal, suggesting the identity of the Mahoning sandstone with the Shamoken conglomerate. A glance at the three sections here compared shows that if the conglomerate of the twelfth Shamoken coal-bed extends west, it is the great Anvil rock of Kentucky, which in the Somerset section assumes its proper place above the Pittsburgh coals, while the Mahoning sandstone is several hundred feet below. There is no good reason why the area of these great sand-rocks should not be coextensive with that of the Bottom Conglomerate, or any other silicious palæozoic formation, the Oriskany or the Potsdam, for example. In this fact, and in the outspread of the limestones and distinguished clays, we have the ground of that pure lithological identification before alluded to, which began naturally to be studied before the palæontological identification could be started, and then had to wait for it, has been outstripped by it, but is following after it and will catch up with it by favor of its guidance. These fossil determinations have indeed a finer faculty; but, after all, they cannot replace structural determinations, and, in fact, depend upon these for their origin, and for their last certainties.

It also appears from these three sections, that our notions of the thickness of the coal formation must be modified. The extraordinary comparative depth of the anthracite coal-measures is all a mistake. This notion originated with the idea of a profound fault running through Pottsville, along the base of the Sharp mountain, leaving a certain number of vertical beds on its south side, and throwing down an enormous additional number on its north side. The sinking of the Salem Vein slopes has shown all this to be false. There is no dislocation. Consequently the number of beds cannot be materially greater on one side of the synclinal than on the other. Mr. Peter W. Shaeffer, the geologist, thinks he has proven by structure that the Gate, Salem, Lewis, and Spohn are the same bed. Mr. Lesquereux asserts the same fact from their fossils, and cannot yet find the Pittsburgh bed at Pottsville. The Dauphin section presented with this paper is collated from three old sections by Taylor, and shows three great beds just in the proper place for the Pittsburgh series, in Somerset county, and in Kentucky. In the Broad Top, Frostburg, and Shamoken basins the measures are equally deep. The inference is indubitable. There is no impoverishment of the carboniferous westward. The First Report of the Kentucky Survey has doubled the column, but there still remain in Kentucky the same thousand feet of strata, containing the same coal-beds as in the anthracite basins of the east. The number of beds — the intervals — are substantially the same; and, what is of more importance, the relative values of the beds among themselves seem to be maintained. In the Somerset section there is almost as fine an exhibition of coal in the upper group as any in the anthracite field, and a finer one than in the Dauphin section. The limestones disappear from the anthracite beds, but occur everywhere else.

Instead of letting any longer our geological glances wander uneasily over these vast areas, to see nothing but a confused intercalation of lenticular deposits, each one limited to a small territory, or so irregular as to defy detection under its many disguises, we are destined to see a fair panorama of harmonized sections, showing the essential regularity and persistency of the few beds of coal and lime, iron, clay, and sand, which constitute a nearly equal thickness of the formation throughout, and producing upon us an effect analogous to that produced upon us by the palæozoic series of formations as a whole, — excepting in the one point of total thickness, which in the palæozoic series, as a whole, undoubtedly declines rapidly westward, but in the coal-measures, taken as a separate formation, does not.

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4. On the Ancient Mining Operations of Lake Superior. By Col. Charles Whittlesey, of Cleveland, Ohio.

(Abstract.)

A remarkable object in the geography of Lake Superior is Point Kenenaw, extending out into the lake some seventy miles, in form like the beak of an eagle. The spine of this peninsula is elevated some five or six hundred feet, composed of copper-bearing rock, which extends to an aggregate length of 160 miles. In very ancient periods of time, mining was carried on extensively along this range, and excavations have been found two miles long, and from twenty to thirty feet deep. This mining was by open cuts, like quarries, for the miners had no means of penetrating deeply into the earth, nor of raising large masses had they so penetrated. The copper found there is well known to exist in its native form, and in masses from minute specimens to those of five hundred tons. The implements and tools made by them of this metal were constructed without melting it, by simply beating it into form in its cold state, and there are no signs whatever that they had any idea of melting copper. Their mode of mining was to build fires to soften the rock, and then break out the pieces of metal by means of large stone hammers. Masses of five or six tons are found, from which pieces have been beaten, and the rest left in place.

At the Minesota mine, those miners had gone down in one place twenty-five feet, and recently, upon opening their excavation, a mass was found, which had been raised upon skids by means of wedges, weighing six tons. The opening had been filled up by rubbish as the miners advanced, and thus, with the accumulation of centuries in addition, it was not easily perceptible. Trees of full growth covered it when again found, and the proof that this was a real excavation for mining purposes was afforded by burnt wood, stone hammers, and the like. This is a sample of those ancient works scattered along a line of 150 miles. Some few cavities have been found, like a room, in the faces of rocky bluffs — places where a dozen men could easily work.

The miners' tools were hammers of hard stone, five to twenty-five pounds in weight; wooden shovels of cedar, used for scraping dirt, as shown by the form to which they are worn; copper implements, like the quarryman's gad, to be used as wedges, driven by the stone mauls; spear heads, copper knives, chisels, and the like. Upon blocks of timbers are still to be seen marks of these axes or chisels.

These tools, and the marks they have left, give a hint toward proving the connection of the people who mined copper here with the ancient Mexicans; for the copper tools, as depicted in Squier and Davis's work, correspond to those of Lake Superior. When I first saw the cuts upon the timber at the lake, I saw at once that they must have been made by an instrument like the copper axe of the Ohio mounds. Another fact was noted, that in the copper tools found in Ohio are seen spots of native silver — a well-known fact in relation to lake copper — and this proves that the Ohio people of the Mound epoch did not possess the art of smelting copper — else the silver would have been melted — and that they did derive their copper from the lake. Now Squier's work seems to prove the Mexican and Ohio race of people the same; the forms of their mounds, pyramids, etc., being the same, lead to this inference; and these facts equally tend to show the identity of the latter with the miners of Lake Superior.

In regard to the period when the mining operations were in progress, it is said that the timber growing in the old excavations is of full size and age. I have counted 290 rings of annual growth. But this does not carry us back so far as we can go with safety. There is plenty of evidence that these ancient trees are of the second growth, at least, since the mines were deserted. This carries us back at least to six hundred years. It must, however, not be forgotten, that the same species of tree does not immediately succeed, but others take its place. The miners, of course, must have cut the timber, and thus we may with confidence carry back our date to a period at least a thousand or twelve hundred years. Again, judging from the amount of work done, and their want of facilities, their labors must have extended through a period of five hundred years. From the fact that no remains of houses in that severe climate are found, no roads nor other improvements made by permanent inhabitants, the conclusion seems inevitable that the mines were wrought only in summer, and then by some people who came thither for the purpose, and departed with the approach of winter. This people, I am of opinion, dwelt in Ohio. The mounds and the mines are of the same age. The Indians of our era could not have been the miners, nor have they any traditions whatever relating to the mines.

Another gentleman has described an ancient pit on Portage lake, twelve to fifteen feet deep, where chisels, many stone hammers, stones used for sharpening gads, a skid charred by fire, etc., were found. In this pit, a tree with four hundred annual circles was growing. He concurred fully in my conclusions.

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5. On the Division of the Azoic Rocks of Canada into Huronian and Lawrentian. By Sir William E. Logan, of Montreal, Canada.

The sub-Silurian Azoic rocks of Canada occupy an area of nearly a quarter of a million of square miles. Independent of their stratification, the parallelism that can be shown to exist, between their lithological character and that of metamorphic rocks of a later age, leaves no doubt on my mind that they are a series of very ancient sedimentary deposits, in an altered condition. The further they are investigated, the greater is the evidence that they must be of very great thickness, and the more strongly is the conviction forced upon me that they are capable of division into stratigraphical groups, the superposition of which will be ultimately demonstrated, while the volume each will be found to possess, and the importance of the economic materials by which some of them are characterized, will render it proper and convenient that they should be recognized by distinct names, and represented by different colors on the geological map.

So early as the year 1845, as will be found by reference to my Report on the Ottawa district (presented to the Canadian government the subsequent year), a division was drawn between that portion which consists of gneiss and its subordinate masses, and that portion consisting of gneiss interstratified with important bands of crystalline limestone. I was then disposed to place the lime-bearing series above the uncalcareous, and although no reason has since been found to contradict this arrangement, nothing has been discovered especially to confirm it; and the complication which subsequent experience has shown to exist in the folds of the whole (apparent dips being from frequent overturns of little value), would induce me to suspend any very positive assertion in respect to their relative superposition, until more extended examination has furnished better evidence.

In the same Report is mentioned, among the Azoic rocks, a formation occurring on Lake Temiscaming, and consisting of silicious slates and slate conglomerates, overlaid by pale sea-green or slightly greenish-white sandstone, with quartzose conglomerates. The slate conglomerates are described as holding pebbles and boulders (sometimes a foot in diameter) derived from the subjacent gneiss, the boulders displaying red feldspar, translucent quartz, green hornblende, and black mica, arranged in parallel layers, which present directions according with the attitude in which the boulders were accidentally inclosed. From this it is evident that the slate conglomerate was not deposited until the subjacent formation had been converted into gneiss, and very probably greatly disturbed; for while the dip of the gneiss, up to the immediate vicinity of the slate conglomerate, was usually at high angles, that of the latter did not exceed nine degrees, and the sandstone above it was nearly horizontal.

In the Report transmitted to the Canadian government in 1848, on the north shore of Lake Huron, similar rocks are described as constituting the group which is rendered of such economic importance, from its association with copper lodes. This group consists of the same silicious slates and slate conglomerates, holding pebbles of syenite instead of gneiss, similar sandstones, sometimes showing ripple-marks, some of the sandstones pale-red green, and similar quartzose conglomerates, in which blood-red jasper pebbles become largely mingled with those of white quartz, and in great mountain masses predominate over them. But the series is here much intersected and interstratified with greenstone trap, which was not observed on Lake Temiscaming.

These rocks were traced along the north shore of Lake Huron, from the vicinity of Sault Ste. Mary for 120 miles; and Mr. Murray ascertained that their limit on the lake shore occurred near Shibahahnahning, where they were succeeded by the underlying gneiss.

The position in which the group was met with on Lake Temiscaming is 130 miles to the north-east of Shibahahnahning, and last year Mr. Murray, in exploring the White-fish river, was enabled to trace the outcrop of the group, characterized by its slates, sandstones, conglomerates, greenstones, and copper lodes, for sixty-five miles from Shibahahnahning to the junction of the Maskinongé and Sturgeon rivers, tributary to Lake Nipissing. The general bearing of the outcrop is north-east, and an equal additional distance in the same direction would strike the exposure on Lake Temiscaming. In the portion which Mr. Murray examined last year, the dip appears to be about north-west, often at a high angle, while that of the subjacent gneiss is more generally south-east, sometimes at a low angle, and in some places nearly horizontal.

To the eastward of this outcrop, Canada has an area of 200,000 square miles. This has yet been but imperfectly examined, but in so far as investigation has proceeded, no similar series of rocks has been met with in it; and it may safely be asserted that none exists between the basset edge of the Lower Silurian, and the gneiss from Shibahahnahning to the Mingan islands, a distance of more than one thousand miles, and probably still further to Labrador.

The group on Lake Huron we have computed to be about 10,000 feet thick, and from its volume, its distinct lithological character, its clearly marked date posterior to the gneiss, and its economic importance as a copper-bearing formation, it appears to me to require a distinct appellation, and a separate color on the map. Indeed, the investigation of Canadian geology could not be conveniently carried on without it. We have, in consequence, given to the series the title of Huronian.

A distinctive name being given to this portion of the Azoic rock, renders it necessary to apply one to the remaining portion. The only local one that would be appropriate in Canada is that derived from the Laurentide range of mountains, which are composed of it from Lake Huron to Labrador. We have, therefore, designated it as the Laurentian series.

These local names are, of course, only provisional, devised for the purpose of avoiding paraphrastic or descriptive titles, the use of which had been found inconvenient, and they can be changed when more important developments, proved to be the equivalents of the series, are met with elsewhere.

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6. On the Probable Subdivision of the Laurentian Series of Rocks of Canada. By Sir William E. Logan, of Montreal, Canada.

I have already indicated the probable separation of the Laurentian rocks of Canada into two great groups, that characterized by the presence of much lime and that without; but from recent investigation, the result of which has just been reported to the Canadian government, it appears to me almost certain that the former of these two great groups will be capable of subdivision, and that some of its bands of limestone, and their associated strata, are of sufficient importance to be represented separately on the map.

Having followed out one of those bands of limestone through all its windings for a distance of eighty miles, the object of the present paper is to exhibit to this section of the Association its geographical distribution, and the forms it presents in the physical structure of the region which it characterizes.

What at first appear to be two bands of these limestones emerge from beneath the Lower Silurian series in the township of Grenville on the Ottawa, and run into the interior parallel to one another, striking N. N. E. They are about two miles separated from one another, and both, with the gneiss between, dip in one direction, that is, N. N. W., at angles varying from about fifty to seventy degrees. Attaining the rear of the township, a distance of about ten miles, the two bands unite, and are found really to constitute but one; the thickness of which, as far as I can make it out, is from five hundred to one thousand feet.

It is plain from this distribution, that the limestone is part of the outcrop of an undulating sheet, the ridges of which have been worn down. But in the horizontal section of an undulating surface, similar forms in the distribution of the rim may be derived from the anticlinal or synclinal part of the undulation, and as the dips on the opposite sides are both one way, it is a question to which part the area belongs.

Within a short distance of the eastern side of the limestone, in fact touching it in one place, an intrusive syenite makes its appearance, belonging to a mass which occupies about thirty square miles in the township of Grenville and Chatham, and runs to a point in Wentworth.

The intrusion of such a mass of igneous rock as this can scarcely fail to have had a considerable effect in modifying the attitude of the strata which surround it. The crystalline condition of the syenite shows that it was slowly cooled under great pressure, and we cannot now say whether it was a deep-seated part of an outburst which reached the surface, as it was then constituted, or whether it was originally overlaid by masses of gneiss and limestone which have since been worn away. In either case the probability is, that it would give to the strata now surrounding it an anticlinal form. It seems probable, therefore, that the western dip, belonging to the eastern band of limestone, where it approaches the syenite, is a true one, and that the form between the bands is synclinal. This appears to be corroborated by the fact that where transverse valleys occur between them, the wearing down of the intermediate gneiss widens the calcareous bands, particularly the east one, and narrows the interval.

The calcareous sheet having thus the form of a trough, the western dip of the western outcrop must be an overturn; and two spurs of the rock which point to one another, the one turning south from the western belt, and the other north from the eastern, must constitute a subordinate anticlinal. Without reference to minor corrugations, the general form of the area would be that of two troughs joined together, each about a mile and a half wide, with an overturn dip on the west side, the one trough running north and south, and the other, as far as unconcealed by the superior fossiliferous strata, south-south-west and north-north-east.

The opposite sides of this calcareous trough run in two valleys, which unite at its northern extremity. But though the limestone then crops out, the valley continues northward into Harrington, and after a short interval shows an isolated patch of limestone of about a mile and a half in length by a mile in breadth, possessing, of course, a synclinal form.

Beyond this the valley splits into two, and while one branch runs rather north of N. E., the other turns north of E. Each of these valleys is paved with limestone, the distribution of which shows a continuation of the synclinal form with a bend more to the eastward than before. The calcareous band on the western side has been traced to the north boundary of the township of Harrington, whence it crosses into Montcalm. It there appears to turn to the westward, but it has not yet been further accurately examined.

The eastern branch has been followed for between six and seven miles into Wentworth, when it appears to turn upon an anticlinal axis, and, proceeding in a bearing S. S. W. for seven miles, it attains the southern boundary of the township, close upon the east side of the northern prolongation of the intrusive syenite. It runs in the same bearing for about three miles along the eastern side of this into Chatham, and becomes deflected to the south-east by the main body of the syenite, to which it runs parallel for about three miles.

It then folds upon the axis of a synclinal, and, running N. N. E. for upwards of five miles, returns into Wentworth, where it gradually bends round more to the eastward, and in about five miles reaches a position in the Gore of Chatham. It here folds over upon the axis of an anticlinal, and turning S. S. E. it maintains this course for about eight miles, in which it crosses into the Seigniory of Argenteuil and reaches the vicinity of Lachute, where it once more bends upon a synclinal axis, and proceeding eastward for about a mile, plunges under the Potsdam sandstone and is lost.

In the winding course derived from the plications of the strata, the limestone usually presents a valley on the geographical surface; but to the west of all the folds that have been described, a bold ridge of gneiss runs from the front of Grenville to the rear of Harrington, the distance being about twenty miles, and the bearing N. N. E. On the west side of this ridge, about midway of its length, there are two areas about five miles long and three quarters of a mile broad, presenting the forms of valleys, which are underlaid by limestone so distributed as to render it probable that they are two outlying parallel troughs joined together, belonging to the same calcareous sheet as the one described.

There would thus be four main synclinals and three main anticlinals; and the breadth they occupy altogether is about eighteen miles, giving about four and a half miles for the breadth of each undulation.

Bands of dolomite sometimes accompany the limestone, which is often interstratified with bands of quartzite. The quartzites appear to be heaviest near the junction of the limestone and the gneiss, becoming thinner and less frequent as you recede from the calcareous rock. The greatest mass of quartzite met with had a vertical measure of four hundred feet, and it was in stratigraphical position beneath the limestone. The quartzite and the gneiss on each side of the limestone are often very thickly studded with garnets, and in some cases the aggregation of these is so close as to constitute a granular garnet rock.

In the Gore of Chatham a band of limestone, about three quarters of a mile to the north-west of the one described, has been traced running parallel with it for seven miles. If the form which has been attributed to the first band be correct, the second would overlie it with a great mass of gneiss between.

A third band of limestone occurs about six miles north of the second; this has been traced for about four miles, running east, which would be nearly parallel with the bearing of the second. In this bearing it has not yet been followed further than to within a short distance from the line between the Seigniory of Argenteuil and the township of Abercrombie, towards the rear of both. Continuous exposures of limestone have been met with on the west side of the Rivière du Nord, at St. Jerome. They have been followed for two miles in a north bearing, and the strike of the stratification, between St. Jerome and the rear of Abercrombie, is such as to make it probable that the St. Jerome rock will ultimately prove to be a part of the third band.

A feature common to both localities is the occurrence immediately near the limestone of immense masses of lime feldspar. North of the Argenteuil band, eight miles, examined across the stratification, consist almost entirely of it, in the form of labradorite, of which masses of the opalescent variety are in some parts inclosed in a paste of the mineral without any play of colors, and they are accompanied by hypersthene and ilmenite. Lime feldspar is abundant at St. Jerome, and its stratified character is conspicuously displayed, the beds running parallel with the limestone.

Mr. Hunt has traced a band of crystalline limestone for eleven miles, running diagonally across the township of Rawdon, in a north bearing. On the west side of this, lime feldspar forms the great bulk of the rock exposures for twelve miles across the measures, and shows a well-marked stratification. It appears probable that the Rawdon calcareous band is the same as the St. Jerome band, and that a synclinal axis exists between the two, the turn of the calcareous band on which is covered up by the fossiliferous rocks to the south.

In Chateau Richer, below Quebec, a band of limestone occurs about a mile from the fossiliferous deposits, and to the north-west of it, lime feldspars present a breadth of eight miles. On an island near Parry's Sound, on Lake Huron, Dr. Bigsby observed the occurrence *in situ* of the opalescent variety of labradorite, and the name of the mineral reminds us of the existence of the rock beyond the eastern end of the province. It thus appears probable that a range of the rock will be found, winding irregularly from one end of the province to the other, of sufficient importance to authorize its representation by a distinct color on the map, and a distinct designation in geological nomenclature.

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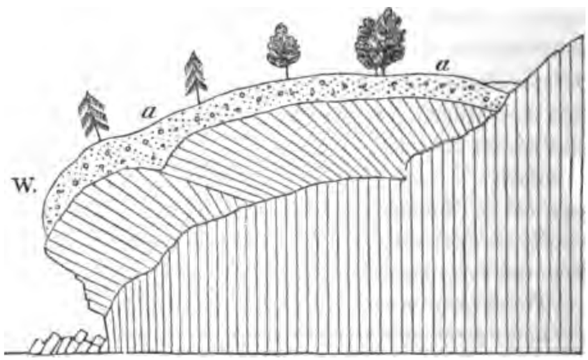
7. On Fractured Ledges of Slate in Vermont. By Charles H. Hitchcock, of Amherst, Mass.

While engaged in the geological survey of Vermont, the past summer, several cases of fractured ledges of slate came under my observation. I will describe them. The slate is argillaceous. Like most of the Vermont rocks, the strike of the slate is about N. 10° E.; the dip varies from 76° E. to 76° W., being in every case nearly perpendicular. The fractures are upon the tops and sides of hills. As the hills trend in the same direction with the strike, these fractures, being made by a force at right angles to the strike, are either upon the east or west side of the hills. Imagine, then, a valley running about north and south, with one of its sides precipitous. This precipitous side may be from twenty to one hundred feet high. The slope of the hill, when examined, as when preparations are made for quarrying roofing slate, is found to be made up of fragments of slate symmetrically arranged, possessing a constant strike, with a dip varying from horizontal to perpendicular. Beneath these fragments, the slate is found in place, undisturbed, in perpendicular strata. The figure will illustrate. A vertical section from east to west, crossing the fragments, is represented.

W.  E.

There are twelve cases of these fractures which have been seen in Vermont. They are in the towns of Guilford, Brattleboro', Dummerston, Northfield, and Newbury. The fracture at Bruce's quarry, in Guilford, has acquired some celebrity from the figures and descriptions of Dr. Jackson and Prof. Hitchcock. The fractured portion of the slate is upon the west side of a hill, the force having come from the east. The mass of broken fragments, so far as exposed, is about one hundred feet, north and south, in length; and its width, from east to west, about forty feet. There has been some alteration in the appearance of these fragments since their first notice, as part have been removed by workmen, and new additions have been made to the pile. Hence the earlier figures do not represent the mass as it appears now.

In the south-east part of Brattleboro', the strike of the slate is N. 20° E. The strata are perpendicular. The force came from the east, as before. The dislocation extends only three feet downwards.

In Northfield, as I was passing on the Vermont Central Railroad, I could see the relics of a similar fracture, the pieces having been mostly removed in the construction of the road. The strike of the slate is N. 20° E.; the dip, 76° W. The force in this example came from the west; and as the rock dips to the west, the breaking was performed more easily than in the previous examples mentioned.

At the slate quarries in another part of Northfield, the strata are simply bent fifteen to twenty degrees from their original position, and this differs altogether from the other examples. The valley runs east and west; the strata cross at nearly right angles, and the bent strata lie in the valley. The force came *down* the valley, much in the manner of a glacier. Some of the cases in Wales are said to be situated like this; but there the breaking was caused by true glaciers. Not so in Northfield, because the evidences of former glaciers, which we find in other valleys in Vermont, are wanting here. Also, had a glacier existed here, other upright ledges, a few rods distant, would have been fractured similarly.

Two cases are in Dummerston, at Clarke's quarry. The direction of the strata is N. 23° E.; dip, 78° W. They are upon the west side of the hill, fractured by a force from N. 15° W., the valley running north and south. The broken mass is about thirty feet wide. A third case nearly covers an extent of several yards, and extends to the depth of two feet.

But the finest example is at Newbury. The track of the Connecticut and Passumpsic Rivers Railroad passes through it, and so great is the mass of broken rock, that for a long time workmen have almost uselessly attempted to diminish the amount, that the trains may pass without inconvenience, else the jar of a heavy train would set in motion a terrific slide. The Connecticut river flows at the base of the mass, two miles above Newbury Centre. The strike is N. 40° E.; dip 76° W.; and the force came from the north-west. The north and south extent of the broken mass is 170 feet. Above the track of the railroad, it extends seventy-five feet, and probably twenty feet more beneath. The width east and west, without actual measurement, I should judge to be forty feet. The rock receives a little hornblende into its composition, thus making tough strata, while at a similar fracture, half a mile south, the rock is soft, and is a talcose slate. Probably for a mile or two along this hill these fractures occur, all of them upon the eastern slope, extending to the apex of the hill.

Having described the appearances, it remains to notice the theories already presented, to account for the facts.

In the First Report on the Geology of New Hampshire, Dr. C. T. Jackson figures and describes the example at Bruce's quarry, in Guilford, and accounts for it by supposing an overlapping of a portion of the strata, succeeded by a lateral thrust occasioned by original formation. His figure was very erroneous. Prof. Hitchcock having shown that the strata beneath had not been disturbed, and that there had been no crossing or overlapping, and that evidence of a lateral thrust was entirely wanting, Dr. Jackson, upon a reëxamination, abandoned the figure and the theory, and gave a correct figure with a new explanation. He proposed the agency of water, percolating between the strata of slate, as the cause of the fracture. Frost enters deeper every year, causing the layers to expand, diverging upwards. A continuation of this process, Dr. Jackson argues, would at length bend over strata enough to equal in size the masses described.

But this theory, correct as far as it goes, does not account for the *breaking off* of the strata; and it would be difficult for expansion simply to throw the strata down loosely into a horizontal position near the top of the hill, before the slate was quarried.

Another case of slate, bent like the letter Z, by the same force, as described by Prof. Adams, shows that there must have been a force *besides* the expansion, directed downwards.

Another theory supposes that the drift agency produced these fractures. There are cases in other parts of the country where drift has without doubt performed the work, as in eastern Massachusetts, New Jersey, and especially the case near Niagara Falls, described by Prof. Hall. But many of the forces in the examples in Vermont came from the east, a direction different from the drift current all over the State. And the fractures, where the force came from the west, are universally situated on the eastern slope, below the top of the hill, thus being precisely on the lee side, where the drift agency has not operated. I can say this with confidence, as I have not found a single case in the State where the south side of a hill or mountain has been worn or scratched by drift. Nor can we suppose, with Darwin, that when the continent was submerged, the slate was fractured by icebergs lifted up and down by the tides; or that huge icebergs, rising and falling by the undulations of a heavy sea, struck these hills with a downward force. The objection rises from the direction. The current of the ocean in the drift period was about north and south. The onward southern movement of the iceberg, then, would crush the strata so obliquely, that its mark — which is wanting — might be seen at the present day. And it would be very curious, also, to know how an iceberg, in these twelve cases, always happened to strike upon the slope of the hill, either eastern or western, and never upon the top of the hill.

To suppose that these effects are attributable to glaciers, would be inconsistent with the facts; since the glaciers would have followed the slopes of the valleys, a course at right angles to that taken by these forces. Upon visiting one of these fractures recently, the true explanation flashed over the mind. We stood upon the top of the crushed portion, and saw that we were upon the debris of a veritable slide, which formed during the past winter. The water percolating into the seams of the rock had frozen, separating the strata. Gradually the strata got upon their edges, perhaps requiring a hundred years, when they were ready to be broken by any force pushing laterally. In the winter the hill is covered with snow, which, in a severe season like the last, accumulates in enormous masses. This, with the loose soil and timber resting upon the weakened strata, together with the weight of the strata themselves, upon a slight impetus would begin to slide down the hill, when the strata previously made perpendicular by the freezing water will be broken off, the top of the severed mass will be dislocated the most, even to horizontal, and rarely back to perpendicular (see figure, in which *aa* represents the slide). Thus the work of fracturing is going on at present under favorable circumstances. The case alluded to occurred only during the past winter, and we were so fortunate as to visit the quarry before the workmen had removed the fragments.

This, we trust, presents the true cause of the phenomena. It seems much better to seek for forces now in operation to explain this and all phenomena, than to attribute causes which may or may not have operated in any particular district.

8. On a Feature in the Range of the Western Mountain System of North America. By Major W. H. Emory, U. S. A., of Washington, D. C.

At the last meeting of the American Association for the Advancement of Science, the undersigned read a paper giving a general view of that portion of the American continent traversed by the United States and Mexican Boundary line, and of the lines previously explored by him. Since then the projection of the local surveys has developed what seems to be a law in regard to the orography of that portion of the continent, which it is thought may be general throughout the western portion of the continent, when the surveys in the interior are made with the same minuteness as those on the boundary.

Most of the surveys and explorations across the continent have been made for the purpose of finding the best route for a railway or wagon road, or for the purpose of ascertaining the sources, direction, and capacity of rivers for navigation, and they have usually followed the valleys and cañons of the mountain system. Hence, what was seen and projected by the persons conducting these surveys, were either valleys surrounded by mountains or mountain passes. The mountain system itself was not traversed transversely by these surveys, and the topography connecting the different points, where the mountains were crossed, were represented hypothetically. Hence the variety of forms the system of mountains assume with every new map issued of the American continent.

The line of the Boundary Survey was, however, in a direction very generally at right angles to the meridian, and in a straight line across the mountains, without regard to valleys and passes. The projection of these surveys develops the singular fact, that the various chains of mountains crossed, although presenting to the traveller and casual observer apparently a continuous line, are in fact composed of a variety of isolated chains disposed *en echelon*.

To illustrate the subject more clearly, I present two proof sheets of maps yet unfinished. A glance at them will show better than written description the orographic law, which I have attempted briefly to explain.

The discovery of this form of the mountains will go far to explain, what has heretofore appeared so extraordinary to casual observers, that the great rivers of the western portion of the American continent should almost universally strike the great chains of mountains nearly at right angles to their general direction. It now appears, that, with regard at least to those mountains examined, there are valleys between the links forming the chain, and these valleys are sometimes lower than the plains between the different ranges of mountains.

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9. On the Crystalline Rocks of the North Highlands of Scotland. (In the form of a letter addressed to Sir William Logan.)

London, July 27, 1857.

My dear Sir William,—Being unable, to my great regret, to attend the Montreal meeting of the American Association for the Advancement of Science, where my distinguished friend, Professor Ramsay, will represent British geologists and our survey, I beg to communicate to you, and any geological contemporaries who may be present, the final determination of a question which has been much agitated in this country, and which has just been settled by a comparison with North American typical fossils of Lower Silurian age. This question is:—What is the true place in the geological series, of those great masses of crystalline or subcrystalline stratified rocks, in the North Highlands of Scotland, in some of which organic remains were discovered by Mr. Charles Peach, in 1855?

That discovery induced me, in the same year, to revisit the localities in the north-west part of Sutherlandshire, to the east of Cape Wrath (Durness), in which the fossils had been detected; my chief object being to ascertain if the views of former explorers of that region, including Sedgwick and myself in 1827, were correct; namely, that these quartz rocks and limestones, associated with mica schist and a sort of gneiss, are of a more ancient date than the great series of Old Red Sandstone, or Devonian deposits, that occupy so large a portion of the north-east of Scotland, and are particularly developed in Caithness and the Orkney islands.

The results arrived at in that excursion, in which I was accompanied by Prof. James Nicol, were communicated at the meeting of the British Association, at Glasgow, in September, 1855, and published in the volume of that year — (See Transactions of the Sections, 1855, p. 85). I then reaffirmed the opinions I had formed in the year 1827, in company with Professor Sedgwick, as to the anteriority of all these quartz rocks, with intercalated limestones, to the Old Red Sandstone, or Devonian System; and judging from the facts that such crystalline and subcrystalline strata reposed unconformably upon an ancient granitoidal gneiss, and were flanked and surmounted by the ichthyolitic deposits of Caithness, I expressed my belief, that, although very imperfect and difficult of absolute determination, the fossils there found by Mr. Peach were of Lower Silurian age.

At that time, my eminent and lamented friend, the late Hugh Miller, had suggested, theoretically, that the quartzites and limestones of the north-western Highlands might prove to be the metamorphosed equivalents of the Old Red series of the Eastern Coast; and, subsequently, Prof. Nicol has even endeavored to show that these rocks may be the metamorphosed representatives of the carboniferous series of the south of Scotland! Both these suggestions were, of course, opposed to my own belief, and as they have been put forth by distinguished contemporaries, I have now to show how my own views have been sustained.

Within these few weeks, Mr. C. Peach has found, in the same locality, (Durness,) other and better preserved fossils, which have, I rejoice to say, set the *questio vexata* at rest, as will be seen by the annexed note of Mr. Salter, who unhesitatingly compares these remains with those known to Mr. James Hall, yourself, and other North American geologists, as occupying the true Silurian position of the calciferous sandrock and base of the Trenton limestones.

It is of course most gratifying to me to find that the general views of succession of the rocks of my native Highlands, indicated so far back as the years 1826–7, — opinions then formed irrespectively of zoölogical evidences, and simply from the physical relations of the rock masses, — should have been thus supported by fossil discoveries.

North American geologists will, of course, have no difficulty in understanding and admitting the conversion of Lower Silurian sediments into quartz rocks, crystalline limestone, mica-schists, chloritic slates, etc.; since their own eastern coast ranges exhibit such phenomena, some of which have been described and mapped by yourself.

To the geologists of the old country this determination is of the deepest interest; for it gives them a key to unravel the real age of large masses of the quartzites, limestones, chloritic and clay slates, mica-schists and quasi-gneissic rocks, (sometimes more, sometimes less metamorphosed,) which occupy vast wild tracts of the Highlands of Scotland.

The general order of the Scottish rocks is, therefore, pretty well ascertained. The lowest known rocks are masses of granitoid gneiss, on the upturned edges of which repose certain hard gritty beds and conglomerates, often of a red color, which, in the early days of our science, were confounded with the Old Red Sandstone. Now, however, that the existence of conglomerates at different levels in the Lower Silurian rocks of the south of Scotland has been demonstrated, (See Siluria, pp. 156–60,) the old views dependent on the mineral characters only, have been swept away. The lowest, indeed, of the conglomerates on the north-west coast of the Highlands may pass for the Cambrian rocks of the Geological survey. Then follows in an ascending order, the series of quartzites, mica and chloritic schists, etc., with included limestones, representing in a metamorphic condition the Lower Silurian sediments.

It is highly probable that the Upper Silurian rocks, which exist partially in the south of Scotland, have no real equivalent in the Highland; since the metamorphic rocks above adverted to are unconformably overlapped by those conglomerates and sandstones which form the very base of the Devonian rocks or Old Red Sandstone.

That great series is clearly exhibited on the north-east coast of the Highlands, and is made up of three subdivisions; namely, (a) Lower Conglomerates and Sandstone, (b) Middle Flagstones and Schists, with abundance of the well-known ichthyolites, and (c) Overlying Sandstones, — the latter constituting the northern headlands of Caithness, and the chief hills of the Orkney islands.

I feel confident that this triple series represents in full, as I have endeavored to show in my work (Siluria), the Devonian rocks of Devonshire, as well as the slaty rocks of the Rhenish Provinces (including the Terrain Rhénan of Dumont).

The *experimentum crucis* as respects Russia was in fact settled by the discoveries of my colleagues, De Verneuil and Keyserling, and myself, when we found the fossil shells of Devonshire and of the gorges of the Rhine in the same beds with the ichthyolites of the Scottish Old Red; many species being identical.

In turning to Ireland we have there obtained evidences illustrative of the conversion of Lower Silurian rocks, as shown by sections across the Connemarra mountains, where a great succession of crystalline limestone and quartzites, including the green Connemarra marble, having been observed to lie directly beneath strata with fossils of the Llandovery rocks (Middle Silurian), I have had no hesitation in considering these altered masses to be representatives of the Lower Silurian of other tracts. (See Siluria, p. 108.) *

Again adverting to Ireland, the survey under our friend Mr. Beete Jukes has ascertained, that in the Dingle Promontory true Upper Silurian rocks, with both Wenlock and Ludlow fossils, are conformably surmounted by many thousand feet of hard chloritic and silicious grits and schists (Glengariff grits), which represent, in my opinion, the great mass of the Devonian rocks. The peculiarity, however, of the Irish section is, that between these Glengariff Grits, and that which has hitherto been exclusively called the Old Red Sandstone of Ireland, there is a great hiatus; for the latter reposes on the edges of the former, and passes conformably under the carboniferous deposits.

This phenomenon, however, simply shows that a great break or local change in the sediments took place in the south-west of Ireland which had no existence in the north-east of Scotland, where the Old Red or Devonian series is continuous.

I cannot on this occasion enter into questions of detail concerning the localities where the Upper Silurian strata pass upwards with perfect conformity into the Old Red or Devonian rocks, or indicate other tracts in Europe, (notably in France and Spain,) where on the contrary the Upper Silurian is entirely omitted. In regard to local dislocations, I particularly refer you to my comparison of the Old Rocks of the Thuringerwald and the Hartz.† I will simply conclude this letter by calling your attention to what is now seen to be the true

* I examined this tract last year in company with Mr. Jukes, Mr. Griffith, and Mr. Salter. Mr. Du Noyer has ably mapped and delineated the country.

† Quarterly Journal, Geological Society, November, 1855.

method of comparing the Older Palæozoic or Silurian rocks of distant regions.

When that skilful and profound geologist, M. Barrande, published in the course of last year his most instructive essay, entitled "Parallèle entre les dépôts siluriens de Bohême et de Scandinavie," he showed how with an agreement in generic characters of the fossils of each Silurian zone, thus indicating a general harmony, there was a great contrast in the species of marine animals in each of the countries compared. By applying this method in a different sense, I may now say that when the Silurian rocks are viewed in their extension through the same latitudes, a remarkable specific agreement is clearly traceable. On the other hand, the Silurian fossils of Bohemia are in accordance with those of France and Spain, or along another and distinct broad southern zone of the same age.

The Silurians of Scandinavia are of the British and American type. In making known the description of the Silurian rocks of Norway by Mr. Kejereslf,* I have recently shown how remarkable is the persistence of the Lower Silurian types (even in species) when these rocks are followed from Scandinavia into the British isles, and to how great an extent this resemblance of type is preserved, even when the Atlantic is traversed, and that the same strata in the crust of the globe are again met with in North America. The occurrence in the south of Scotland of the *Maclurea magna* of Hall, of the *Isotelus gigas* in Ireland, and of the fossils of your calciferous sand-rock in our Scottish Highlands, are all most satisfactory proofs that the order in Canada and the country of our kinsmen is, with certain modifications, the same as in the ancient realm of Caractacus.

Excuse this hurried letter, and wishing you as successful a meeting as your labors and those of my other eminent friends in the United States deserve,

Believe me to be,

Yours very sincerely,

RODERICK I. MURCHISON.

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* Journal Geological Society, about to be published.

PALEONTOLOGY.

1. Note on the Fossils in the Crystalline Rocks of the North Highlands of Scotland. By J. W. Salter, F. G. S.

The specimens previously sent from Durness were far from satisfactory, and, though clearly Palæozoic, could not be appealed to as settling their true place. They might, indeed, have been either Carboniferous or Devonian, although Sir R. I. Murchison had offered strong geological reasons to lead us to suppose them to be Lower Silurian forms. One cast in particular, which was at first doubtfully regarded as a *Maclurea*, though it had a right-handed curvature of the whorls, is now more properly referred to *Raphistoma* or *Ophileta*. And an *Orthoceras* present in the same beds could not decide the case. But those lately collected by Mr. Peach leave no doubt as to the true age of the beds. The principal fossil will be particularly interesting to Canadian geologists, being the same as one from the "Calciferous Sand-rock" of Beauharnois, and which, being undescribed, has received the MSS. name of *Ophileta compacta*. The genus is doubtful, and the fossil is probably only a sub-genus of *Raphistoma* (Hall), the species of which have a wide umbilicus (bounded by a very prominent ridge) and straight-sided whorls. This species in Canada grew full an inch and a half wide, and had as many as six or seven whorls, flat above, and with a sunk apex, and a very broad and wide umbilicus, so that the entire shell is much attenuated, and the inner whorls would easily break out, as in Mr. Hall's figure of *O. levata*, Pal. 4, 7; Pl. 3, vol. 1, fig. 4. The whorls of that species are much less carinate below, and the umbilicus not nearly so wide. *O. compacta* will be fully figured and described in a decade of the Canadian fossils; it is unnecessary to say more of it here. It is curious that the *Euomphalus* (*Maclurea matutina*) which accompanies the Beauharnois fossil in Canada, is found also in the Highland beds, with another thick whorled species. Again, a species of *Pleurotomaria*, known in America in the Trenton limestone — the *P. subconica* (Hall) — comes so very near to one of our fossils, that it might well be only a variety of the species. The Highland fossil has rather more numerous whorls, and perhaps a broader band. The genus *Oncoceras*, so characteristic of the Trenton limestone, also occurs, but of a larger species, with more numerous septa than the *O. constrictum*. As the calcareous beds in Canada frequently contain the fossils of more than one subdivision of the New York series, it is not more than we should expect, to find the above fossils associated in a single thick band of limestone. It is most satisfactory to find, in the northernmost part of Scotland, the representatives of the Calciferous sand-rock and the Trenton limestones, as in the south of Scotland, that of the Chazy limestone.* And as the former repose upon a quartz rock with abundance of fucoidal impressions, the suggestion is obvious that such rock may, perhaps, occupy the place of the Potsdam sandstone.

NOTE. Mr. C. Peach is now proceeding, at my special request, to endeavor to collect more fossils, not only at Durness, but throughout the Assynt and other tracts into which the same limestones and quartzites extend.

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2. On Graptopora, a new Genus of Polyzoa, allied to the Graptolites. By J. W. Salter, F. G. S.

It has long been doubtful to what class and order the *Graptolite* belongs. McCoy stands almost alone in referring them to the Sertulariadæ, on account of their being built up, as he supposes, of separate cell, cut off by a diaphragm from the common tube, a structure which Barrande's discoveries have disproved. The generality of naturalists have followed Dr. Beck, in classing *Graptolites* with the Alcyonarian polypes (*Virgularia* being the form with which they are most allied), and the figures of Geinitz and of Richter, showing what appear to have been soft and branched forms, probably belonging to the Graptolitidæ, have tended to confirm such a conclusion.

* The great *Maclurea* of Grivan, in Ayrshire, has been identified with the *M. magna* (Hall) by Prof. McCoy.

Mr. Huxley is the only naturalist, so far as I am aware, who has suggested the Polyzoa as a group to which these bodies might belong. Symmetrically branched forms, connected by a membraneous expansion at the common base, found by Sir W. E. Logan in the Hudson group of Canada, seemed to him to offer analogy with Defrancia and allied genera. But there was no link to connect the horny tubes and long-projecting cells of the Graptolites with any of the forms of Polyzoa, so abundant in the old palæozoic sea. This link, or one of the links, has at length been supplied, and it is in a direction where we should perhaps not have looked for it. The Fenestellidæ have been recognized as an extinct group of Polyzoa, allied to Retepora, and differing from it by the presence of a vertical fibrous layer on the reverse side.

But all the known species are calcareous, furnished with a layer of fibrous structure, above noted, in which are set short cells, quite distinct from each other; so that to convert a Fenestella into a Graptolite, even supposing we have the least branched species of the former and the most complex of the latter to compare, it would be necessary to get rid of nearly all the calcareous material, reduce the fibrous layer to a mere thread (the axis), and lengthen out the cells enormously, while they were made fully to communicate with each other at their bases. The connecting processes, too, or dissepiments, must be removed, as nothing of the kind has yet been observed in the Graptolithnia, unless some obscure traces of lateral processes observed by my friend, Dr. Wyrille Thomson, in the *Grapt. Sedgwicki*, may have relation to them.

The species about to be described (Gorgonia flabelliformis Eichwald) appears to fulfil most of the conditions required to connect the two groups, since it has only a corneous structure, with no calcareous layers, and has the cells elongated on the inner surface of the cup, so that but for the connecting processes, in which it resembles Fenestella, it would be a branched form of Graptolite, not greatly more compound than the species discovered in Canada, and before noticed. I had found it in plenty in North Wales (1853), and named it *Fenestella socialis* before I was aware of its structure or its identity with Eichwald's species.

In a letter from Count Keyserling to Sir R. I. Murchison (1857), he mentions this *Gorgonia flabelliformis*, Eich., as abundant in the lower schists of Russia, as well as Sweden, and that Angelin has proposed the name *Phyllograpsus* for it. It is evident, therefore, the Swedish Paleontologist has observed the affinities above noted; and I would willingly adopt his expressive unpublished name, were it not that we are sure of the affinity with Fenestella, while that with the Graptolites, though strongly suggested by the structure, is not certain enough to warrant our including it in a symmetrical nomenclature with the Graptolitidæ. The term *Graptopora* will be sufficiently in unison with other genera of the Fenestellidæ, and at the same time indicate what I believe a true relation with the Graptolite group.*

GRAPTOPORA–*n–g.*

General form, rooted, net-like, cup-shaped, oblong, parallel, dichotomous branches, connected by very irregularly placed processes (dissepiments). The branches of corneous texture (without a fibrous layer), of a double row of cells, which have projecting angular mouths, on the inner sides of the cup.

G. socialis, n. sp. [*Gorgonia flabelliformis*, Eichw. Urwelt Russlands, Heft 2, p. 45, tab. 1, f. 6 ?]

G. *triuncialis, calycibus tubulosis congregatis* [*ex eadem radice* 3–4 ?]: *ramulis parallelis approximatis; dissepimentes crebris irregularibus.*

Lest the English species should prove distinct from the larger Scandinavian form (*Gorgonia flabelliformis*, Eichwald), a specific character is given above; but they appear to me identical; and, in a collection lately sent by M. Kjerulf from Xtiania, the Swedish species appears with the name (*Fenestella socialis*) which has been for some years in manuscript, applied to the English specimens in our cabinets. I had not, however, discovered, till lately, its generic character.

The English and Welsh specimens differ a little in the width of the meshes; but in no important particular. The cups are frequently

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* There is another reason why I should be glad to see the name adopted. The genus was described, and the name printed, in the second edition of Sir R. I. Murchison's Siluria (now in press), before I was aware that Angelin had noticed it. And I believe his name is only in MSS.; if otherwise, *Graptopora* should of course yield to *Phyllograpsus*.

three inches long and one and a half broad at the top, and pretty regularly conical. They are very generally grouped in threes or fours at the base, and must have grown obliquely, if not radiating horizontally, as they appear now arranged so upon the slabs. The attachment is a short radicle, which soon branches into three or four dichotomising stems, and these again branch three or four times in the lower and middle part of the cup, till the full number (thirty to thirty-five) of parallel stems (or interstices, as they used to be called) are obtained, which end rather abruptly and regularly at the upper margin of the cup.

The branches themselves are minutely flexuous throughout, and throw off on either side numerous thin processes (or dissepiments) very irregular in direction and position (Eichwald especially mentions this irregularity in his *G. flabelliformis*), sometimes oblique upwards, sometimes downwards, and not unfrequently two close together, or partially coalescing. The cells show themselves in a double row in the branches, projecting a little on each side of them, and rather closely set at about the diameter of the branches, apart. (The mouths of the cells only show on parts of the English specimens, but are clearly seen on those from Scandinavia, along the whole length of the branches, which are laterally compressed.) They are projecting angular processes, exactly like the teeth of *Graptolites*, the lower edge oblique and produced upwards into a short spine, the upper edge nearly straight and horizontal, or a very little concave. They are not unlike the cells and mouths of *Didymograpsus* (*Graptolites*) *geminus*, His., a species which occurs, with that above described, in the lowest alum slates of Scandinavia.

The irregularity of the dissepiments is a character, so far as I know, not met with in any of the true *Fenestellidæ*. However, the corneous texture and projecting cells will, I think, separate *Graptopora* from that group, and constitute it a new family, leading off to the Graptolitidæ.

3. On the Varieties and Mode of Preservation of the Fossils known as Sternbergiæ. By J. W. Dawson, F. G. S., of Montreal.

The fossils which have been named Sternbergiæ and sometimes Artisiæ, are usually mere casts in clay or sand, having a transversely wrinkled surface, and sometimes an external coaly coating and traces of internal coaly partitions. They are found in the coal formation rocks of most countries, and very abundantly in those of Nova Scotia. Until the recent discoveries of Corda and Williamson, they were objects of curious and varied conjecture to geologists and botanists, and were supposed to indicate some very extraordinary and anomalous vegetable structure. They are now known to be casts of the piths or internal medullary cavities of trees, and the genera to which some of them belong have been pointed out. Many interesting truths with respect to them, both in their geological and botanical relations, still, however, remain to be developed; and in the present paper I propose to offer some further contributions towards their history, and the geological inferences deducible from it.

In a paper communicated to the Geological Society of London, in 1846, to which Professor Williamson, in his able memoir in the Manchester Transactions,* assigns the credit of first suggesting that connection between these curious fossils and the conifers, which he has so successfully worked out, I stated my belief that those specimens of Sternbergiæ which occur with only thin smooth coatings of coal, might have belonged to rush-like endogens; while those to which fragments of fossil wood were attached, presented structures resembling those of conifers. These last were not, however, so well preserved as to justify me in speaking very positively as to their coniferous affinities. They were also comparatively rare; and I was unable to understand how casts of the pith of conifers could assume the appearance of the naked or thinly coated Sternbergiæ. Additional specimens, affording well-preserved coniferous tissue, have removed these doubts, and in connection with others in a less perfect state of preservation, have en-

* Vol. ix., 1851.

ILLUSTRATIONS OF STERNBERGIA.

Fig. 1.

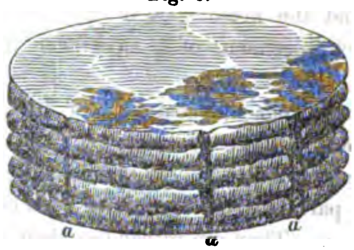

Fig. 2.

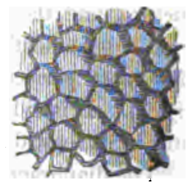

Fig. 1. — Portion of Sternbergia, (nat. size,) (*a*) Remains of Woody Fibre.

Fig. 2. — Transverse Section of one of the Diaphragms of Fig. 1, (magnified.)

Fig. 3.

Fig. 4.

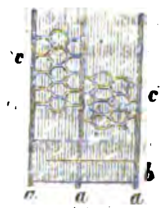

Fig. 3. — Junction of Diaphragm and Wood of Fig. 1, (magnified).

Fig. 4. — Woody Tissue of Fig. 1, (highly magnified). (*a*) Cell Walls. (*b*) Medullary Rays. (*c*) Hexagonal Discs.

Fig. 5.

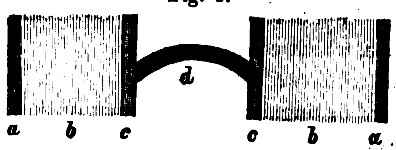

Fig. 5, *A*.

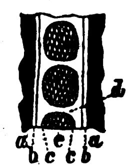

Fig. 5. — Longitudinal Section of Recent Cecropia peltata, (nat. size). (*a*) Bark. (*b*) Wood. (*c*) Pith lining Medullary Cavity. (*d*) Diaphragm of Pith.

Fig. 5, *A*. — Section of young branch of a species of Ficus, showing the outer pith tissue and partitions, and the spaces between the latter still filled with the ordinary or inner pith. Reference letters same as Fig. 5. (*e*) Ordinary Pith.

Fig. 6.

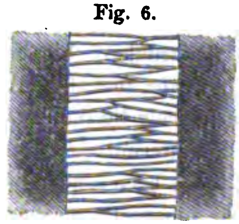

Fig. 7.

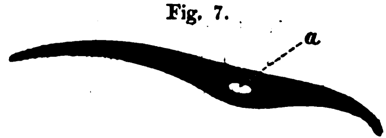

Fig. 6. — Flattened Sternbergia with compressed Bark, (nat. size).

Fig. 7. — Flattened Trunk, one foot in diameter, with Sternbergia. (*a*) Portion of Sternbergia Cast.

abled me more fully to comprehend the homologies of this curious structure, and the manner in which specimens of it have been preserved independently of the wood.

My most perfect specimen is one from the coal field of Pictou, (Fig. 1). It is cylindrical but somewhat flattened, being one inch two tenths in its least diameter, and one inch and seven tenths in its greatest. The diaphragms or transverse partitions appear to have been continuous, though now somewhat broken. They are rather less than one tenth of an inch apart, and are more regular than is usual in these fossils. The outer surface of the pith, except where covered by the remains of the wood, is marked by strong wrinkles, corresponding to the diaphragms. The little transverse ridges are in part coated with a smooth tissue similar to that of the diaphragms, and of nearly the same thickness.

When traced around the circumference or toward the centre, the partitions sometimes coalesce and become double, and there is a tendency to the alternation of wider and narrower wrinkles on the surface. In these characters and in its general external aspect, the specimen perfectly resembles many of the ordinary naked Sternbergiæ.

On microscopic examination the partitions are found to consist of condensed pith, which, from the compression of the cells, must have been of a firm bark-like texture in the recent plant, (Figs. 2 and 3). The wood attached to the surface, which consists of merely a few small splinters, is distinctly coniferous, with two and three rows of discs on the cell walls, (Fig. 4). It is not distinguishable from that of *Pinites*, (*Dadoxylon*,) *Brandlingi*, of Witham, or from that of the specimens figured by Professor Williamson. The wood and transverse partitions are perfectly silicified, and of a dark-brown color. The partitions are coated with small colorless crystals of quartz and a little iron pyrites, and the remaining spaces are filled with crystalline laminæ of sulphate of barytes.

Unfortunately this fine specimen does not possess enough of its woody tissue to show the dimensions or age of the trunk or branch which contained this enormous pith. It proves, however, that the pith itself has not been merely dried and cracked transversely by the elongation of the stem, as appears to be the case in the butternut, (*Juglans Cinerea*,) and some other modern trees; but that it has been condensed into a firm epidermis-like coating and partitions, apparently less destructible than the woody tissue which invested them. In this specimen the process of condensation has been carried much further than in that described by Professor Williamson, in which a portion of the unaltered pith remained between the Sternbergia-cast and the wood. It thus more fully explains the possibility of the preservation of such hollow chambered piths, after the disappearance of the wood. It also shows that the coaly coating investing such detached pith casts is not the medullary sheath, properly so called, but the outer part of the condensed pith itself.

The examination of this specimen having convinced me that the structure of Sternbergiæ implies something more than the transverse cracking observed in Juglandaceæ, I proceeded to compare it with other piths, and especially with that of Cecropia Peltata, a West Indian tree, of the natural family Artocarpaceæ, a specimen of which was kindly presented to me by Professor Balfour, of Edinburgh, and which I believe has been noticed by Dr. Fleming, in a paper to which I have not had access. This recent stem is two inches in diameter. Its medullary cylinder is three quarters of an inch in diameter, and is lined throughout by a coating of dense whitish pith tissue, one twentieth of an inch in thickness. This condensed pith is of a firm corky texture, and forms a sort of internal bark lining the medullary cavity. Within this the stem is hollow, but is crossed by arched partitions, convex upward, and distant from each other from three quarters to one and a quarter inch. These partitions are of the same white corky tissue with the pith lining the cavity; and on their surfaces, as well as on that of the latter, are small patches of brownish large-celled pith, being the remains of that which has disappeared from the intervening spaces. Each partition corresponds with the upper margin of one of the large triangular leaf scars, arranged in quincuncial order on the surface of the stem. (Fig. 5.)

Inferring from these appearances that this plant contains two distinct kinds of pith tissue, differing in duration and probably in function, I obtained, for comparison, specimens of living plants of this and allied families. In some of these, and especially in a species labelled "Ficus Imperialis," from Jamaica, I found the same structure; and in the young branches, before the central part of the pith was broken up, it was evident that the tissue was of two distinct kinds, — one forming the outer coating and transverse partitions opposite the insertions of the leaves, and retaining its vitality for several years at least; the other occupying the intervening spaces or internodes, of looser texture, speedily drying up, and ultimately disappearing, (Fig. 5, *A*).

Another variety of the Sternbergia-like pith structure appears in a rapidly growing exogeneous tree with opposite leaves, cultivated here, and I believe a species of Paullinia. In this trunk there are thick nodal partitions, and the intervening spaces are hollow, and lined with firm corky pith, with its superficial portion condensed into a sort of epidermis, and marked with transverse wrinkles; a cast of which would resemble those Sternbergiæ which have merely wrinkles without diaphragms.

The trunks above-noticed are of rapid growth, and have large leaves; and it is probable that the more permanent pith tissue of the medullary lining and partitions serves to equalize the distribution of the juices of the stem, which might otherwise be endangered by the tearing of the ordinary pith in the rapid elongation of the internodes. A similar structure has evidently existed in the coal-formation conifers of the genus Dadoxylon, and possibly they also were of rapid growth, and furnished with very large or abundant leaves.

I have no means of ascertaining to what extent this structure may characterize certain botanical families, nor what gradations it may present, between the mere transverse cracking observed in the trunks of the Butternut and other Juglandaceæ, and the perfect partitions developed in Cecropia. Prof. Gray states that the transverse pith structure is characteristic of the North American trees of the genus Juglans, but wanting in the closely allied genus Carya—a parallel case with its apparent restriction to one genus, or perhaps species, of extinct conifers. It is quite possible that some of the more rapidly growing and thicker-branched species of southern conifers still present similar structures. The axes of cones also deserve study in this respect, since I have observed that the pith of the cone of Pinus Strobus shows, though obscurely, a tendency to the formation of transverse dissepiments.

Applying the facts above stated to the different varieties or species of Sternbergia, we must in the first place connect with these fossils such plants as the Pinites Medullaris of Witham. I have not seen a longitudinal section of this fossil, but should expect it to present a transverse structure of the Sternbergia type. The first specimen described by Prof. Williamson represents a second variety, in which the transverse structure is developed in the central part of the pith, but not at the sides. In my Pictou specimen the pith has wholly disappeared, with the exception of the denser outer coating and transverse plates. All these are distinctly coniferous, and the differences that appear may be due merely to age, or more or less rapid growth.

Other specimens of Sternbergia want the internal partitions, which may, however, have been removed by decay; and these often retain very imperfect traces, or none, of the investing wood. In the case of those which retain any portion of the wood, sufficient to render probable their coniferous affinities, the surface-markings are similar in character to those of my Pictou specimen, but often vary greatly in their dimensions, some having fine transverse wrinkles, others having these wide and coarse. Of those specimens which retain no wood, but only a thin coaly investment representing the outer pith, many cannot be distinguished by their superficial markings from those that are known to be coniferous, and they occasionally afford evidence that we must not attach too much importance to the character of their markings. A very instructive specimen of this kind from Ohio, with which I have been favored by Prof. Newberry, has in a portion of its thicker end very fine transverse wrinkles, and in the remainder of the specimen much coarser wrinkles. This difference marks, perhaps, the various rates of growth in successive seasons, or the change of the character of the pith in older portions of the stem.

I have not been so fortunate as to find any of the Sternbergia or Artisia casts associated with the wood of plants allied to Lepidodendron, as observed by M. Corda. There are, however, in the collection of Prof. Newberry, as well as in my own, specimens which present very considerable differences in their external characters from those of the varieties known to have been coniferous, and which may be the axes of such plants.

The state of preservation of the Sternbergia casts in reference to the woody matter which surrounded them, presents, in a geological point of view, many interesting features. Prof. Williamson's specimen I suppose to be unique in its showing all the tissues of the branch or trunk in a good state of preservation. More frequently, only fragments of the wood remain, in such a condition as to evidence an advanced state of decay; while the bark-like medullary lining remains

In other specimens, the coaly coating investing the cast sends forth flat expansions on either side, as if the Sternbergia had been the mid-rib of a long, thick leaf. This appearance, at one time very perplexing to me, I suppose to result from the entire removal of the wood by decay, and the flattening of the bark, so that a perfectly flattened specimen, like that in fig. 6, may be all that remains of a coniferous branch nearly two inches in diameter. A still greater amount of decay of woody tissue is evidenced by those Sternbergia casts which are thinly coated with structureless coal. These must, in many cases, represent trunks and branches which have lost their bark and wood by decay; while the tough, cork-like, chambered pith drifted away to be imbedded in a separate state. This might readily happen with the pith of Cecropia; and perhaps that of these coniferous trees may have been more durable; while the wood, like the sap-wood of many modern pines, may have been susceptible of rapid decay, and liable, when exposed to alternate moisture and dryness, to break up into those rectangular blocks, which are seen in the decaying trunks of modern conifers, and are so abundantly scattered over the surfaces of coal and its associated beds in the form of mineral charcoal.

Some specimens of Sternbergia appear to show that they have existed in the interior of trunks of trees of considerable size. The best instance of this that I have found is that represented in fig. 7, from the South Joggins, and which appears to show the remains of a tree a foot in diameter, now flattened and converted into coal, but retaining a distinct cast of a wrinkled Sternbergia pith.

Are we to infer from these facts that the wood of the trees of the genus Dadoxylon was necessarily of a lax and perishable texture? Its structure, and the occurrence of the heart wood of huge trunks of similar character in a perfectly mineralized condition, would lead to a different conclusion; and I suspect that we should rather regard the mode of occurrence of Sternbergia as a caution against the too general inference from the state of preservation of trees of the coal formation, that their tissues were very destructible, and that the beds of coal must consist of such perishable materials. The coniferous character of the Sternbergiæ, in connection with their state of preservation, seems to strengthen a conclusion at which I have been arriving from microscopic and field examinations of the coal and carbonaceous shales, that the thickest beds of coal, at least in eastern America, consist in great part of the flattened bark of successive generations of coniferous, sigillaroid and lepidodendroid trees, the wood of which has perished by slow decay, or appears only in the state of fragments and films of mineral charcoal. This is a view, however, on which I do not now wish to insist, until I have further opportunities of confirming it by observation.

The most abundant locality of Sternbergia with which I am acquainted, occurs in the neighborhood of the town of Pictou, immediately below the bed of erect calamites described in the Journal of the Geological Society (Vol. 7, p. 194). The fossils are found in interrupted beds of very coarse sandstone, with calcareous concretions, imbedded in a thick reddish brown sandstone. These gray patches are full of well preserved calamites, which have either grown upon them, or have been drifted in clumps with their roots entire. The appearances suggest the idea of patches of gray sand rising from a bottom of red mud, with clumps of growing calamites which arrested quantities of drift plants, consisting principally of Sternbergia and fragments of much decayed wood and bark, now in the state of coaly matter too much penetrated by iron pyrites to show its structure distinctly. We thus, probably, have the fresh growing calamites entombed along with the debris of the old decaying conifers of some neighboring shore; furnishing an illustration of the truth that the most ephemeral and perishable forms may be fossilized and preserved, contemporaneously with the decay of the most durable tissues. The rush of a single summer may be preserved with its minutest striae unharmed, when the giant pine of centuries has crumbled into mould. It is so now, and it was so equally in the carboniferous period.

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4. On the Newer Pliocene Fossils of the St. Lawrence Valley. By Professor Dawson, of Montreal.

The object of this paper was in the first place to notice several fossil shells recently found by the author and others in these deposits, and which did not appear to have been previously observed. The species mentioned were:—

Natica Heros, Say	Beauport.
Natica Grœnlandica, Beck	"
Fusus tornatus, Gould	Montreal.
Fusus harpularius, Couthoy	
Rissoa minuta	Montreal.
Turritella (like *erosa*)	Beauport.
Bulla oryza, Tott	Montreal.
Spirorbis sinistrorsa, Montagu	"
Univalve (perhaps *Menestho albula*)	

Most of these are shells now living on the Atlantic coast of America, north of Cape Cod, and some of them ranging very far north. The paper then referred to the distribution of the various kinds of drift in the vicinity of Montreal, and to the conditions of the sea areas, in which the shells and other marine animals of the Newer Pliocene period existed in the St. Lawrence valley. "Good evidence exists of a sea beach on Montreal mountain, at an elevation of 470 feet above the sea. The sea area corresponding to this beach must have extended to the Laurentide hills and the escarpment of Niagara, and communicated freely with the ocean on the east. On the other hand, there are lower shores of the same period only one hundred feet above the St. Lawrence. These must have belonged to a very narrow prolongation of the present Gulf of St. Lawrence. The conditions of climate, ice, drift, etc., corresponding to these different shores, must have been very diverse."

"Again, in the stratified drift, it is possible to recognize, within a few inches of each other a bed, containing deep sea shells, and others containing species that are littoral, these sea bottoms corresponding to different levels of the land. It is evident that any conclusions with reference to the climate indicated by the marine fauna of these successive beds of marine detritus, must take into account these fluctuations of the sea level, and the changes in animal life consequent on them. Taking these into account, positive and reliable results may be attained, and the study of such districts as the St. Lawrence valley may be made to contribute toward the elucidation of the conditions of life in older formations."

5. Fossils of the Sandstones and Slates of North Carolina. By E. Emmons, of Albany.

The inferences which may be derived from the fossils of the sandstones will be better appreciated by reference to the arrangement and relation of the rocks in which they are found.

In the ascending order, the masses and beds hold the following positions: —

1. Conglomerate, both coarse and fine, the coarser materials of which are derived from the broken-down veins of quartz of the auriferous slates of the Taconic system.
2. Red and gray sandstones, which are generally even grained and firm, but which are often separated by soft, disintegrating strata. The red color is replaced by gray, towards the bituminous slate which contains the coal seams. These masses are three thousand feet thick.
3. Bituminous slate, containing three coal seams, two distinct belts of black band, and one of argillaceous iron ore, fine clays, a few beds of sandstone, of which the most important lies between the upper and lower seams of coal, and is about sixteen feet thick. Towards the top of this mass, the bituminous slates alternate many times with magnesio-calcareous strata, which are mainly destitute of the cypridæ and posidonia, which are so common in the bituminous beds.

The whole mass is about six hundred feet thick. Beds of sandstone succeed the latter mass. The thickness above the slates is not well determined. To the latter succeed three or four beds of conglomerate, alternating with beds of fine blue, unbituminized slate, rich in Cycads and ferns, but which contain no animal remains whatever. Their whole thickness is about forty feet. For several hundred feet, however, the sandstones contain plants closely related to those of the unbituminized slates. Finally, the rock becomes a red sandstone, with soft, meshy, and calcareous layers; the whole, however, terminates in masses of very coarse brecciated conglomerates and sandstones.

The red sandstones predominate, and are very clearly divisible into lower and upper, each of which has its slate. In the upper, the slate is at the bottom and unbituminous, but in the lower, it is mostly bituminous, and is bounded below and above by gray sandstones, and is situated near the top of the mass. These rocks lie in a trough, which runs obliquely across the State, terminating in an apex in Granville county, near Oxford, and on the south, traversing a corner of Union county, and finally passing into South Carolina. From every point or side, the land descends to the outer rim of the sandstones. Their surface, together with a narrow border of the adjacent rocks, is overspread with rounded, loose pebbles, whose beds resemble a modern sea beach. These beds of pebbles may probably belong to the Eocene period. The formation is exceedingly variable in thickness. At Egypt, the Gulf, and Carbonton, or for about twenty-five miles measured along a crooked outcrop, or from Martin Dye's to Tooschec's, all the beds are present. But towards the north-east and south-west from these extreme points the lower masses thin out, so that at Lockville, on Deep river, both the lower sandstone and the bituminous slates, with their coal-beds, have disappeared; the lowest masses consist of the conglomerates and slates of the upper sandstone. These had been mistaken for the conglomerates of the lower sandstone, from which much confusion and perplexity had arisen. The most important fossils belonging to the foregoing series occupy two distinct horizons; namely, the bituminous slates of the lower standstone, and the non-bituminous slate at the base of the upper sandstone. The former contains remarkable fauna, which include both fish and reptiles, and a mammal. The fish are mostly Ganoids, with rhombic scales; but there is a large and important one furnished with circular scales and large plates, and which, from the structure of the circular scale, I have named Rabdiolepis, or *rodded scale*. Another Ganoid I have referred to the genus Amblypterus; it resembles very closely the Gyrolepis, Agassiz, of the Muschelkalk. There are already known six genera, one of which is a Tetragonilepis with large and highly polished scales.

The reptiles belong in part to the Thecodons, and are closely related to those of the Bristol conglomerate, England. Their teeth are not only set in distinct sockets, but the spinal cord had a moniliform structure, the canal through which it passed being distinctly enlarged immediately over the body of each vertebra, in which respect these Saurians differ from all others. One genus which has been discovered belongs probably to another family, inasmuch as it was furnished with osseous dermal plates, unlike those of the Teleosaurus. Besides the foregoing, there is also a Labyrinthodon, which has been named, by Prof. Leidy, Dictyocephalus elegans.

One of the foregoing genera has been referred to the Clepsisaurus of Lea; the other is evidently new, and has been named Rutiodon Carolinensis. The most important of the genera belonging to this formation has been referred to the genus Palaeosaurus of Riley and Scutchbury. Some of the teeth appear to be identical with the one figured by the authors referred to, but there are two or more varieties of form which are yet unknown in the Bristol conglomerate; hence there remains some uncertainty with respect to the correctness of the reference, because the standards of comparison are so few in the English formation.

The most interesting of all the fossils of these beds is the jaw of an insectivorous mammal. It differs, generally, from all mammals hitherto discovered. It is, however, remotely related to the Phascolotherium. It differs in one important particular from the latter, and also from the Amphitherium, in its having no incurvation at base. Hence it is supposed to belong to the pescented insectivora. It might at first be inferred, that this mammal would decide the age of the slates, and lead us to regard them as parallel with the stones field slate of the oolite. But it is evident that such an inference is unwarranted, as it is not even generically related to the foregoing mammals; it really determines nothing, as to age or period, of these slates. We must regard it, however, as the oldest mammal yet discovered, inasmuch as it is associated with the Thecodon Saurians.

The second important horizon of fossils is at least two thousand feet above the former, and furnishes only plants. The plant beds appear to indicate short periods of repose, interrupted by intervals of disturbance, during which the beds of conglomerates were deposited. The direction of the materials does not appear to be changed, for the kind of sediment does not differ materially from that which had been previously accumulating; yet there is probably a slight unconformity between the upper and lower sandstones; for the change from a fine to a coarse sediment indicates a change of level which must result in an unconformity; besides, it is apparent, there is an over extension of the beds which were deposited at this horizon.

The majority of the plants are Cycads, though the most common plant is a Calamites, supposed to be the C. arenaceus. The Equisetum columnare, which is common in the Richmond coal-field, is rare in North Carolina. The plant which I have named *Pterozamites decussatus* is supposed, by Prof. Heer of the University of Zurich, to be *Pt. longifolius.*

Among the ferns, the *Acrostachites oblongus*, Em., appears to be the same plant which has been referred to as the Pecopteris Whitbyensis. Its side veins, instead of being forked, are reticulated, as represented in my North Carolina Geological Report, and in the sixth part of the American Geology. The Pecopteris (Aspidites) bullatus, Bunbury, of the Richmond coal-field is, according to Prof. Heer, the P. Stuttgartiensis, and the P. Carolinensis, Em., is related to the Gutbiera angustiloba, Stern. It becomes G. Carolinensis, Heer. The result which I had come to, on an examination of all the fossils which had been found was, that the upper sandstone is equivalent to the Keuper of Europe. On this question, I am confirmed by the results of an examination of the plants figured in my North Carolina Report, by Prof. Heer, who remarks, in a note which I have received, that certain species, such as the Pterozamites longifolius, Equisetum columnare, and Pecopteris Stuttgartensis, are characteristic of the Keuper and Marves Irisces of Germany, France, and Switzerland; and certain others are closely related to species found in Europe, in the Keuper and lower Lias, but are all different specifically; but there are none which are really Oolitic, either in Virginia or North Carolina.

Such being the result of the examination of the flora of these plant beds, it is plain that it sustains the conclusion which I had arrived at, that the fauna of the bituminous slates and lower sandstone must be infra Liassic; indeed, the evidence which these beds independently furnish is conclusive; for, if none of the Saurians can be referred to genera which are known to be Liassic, but must be referred to the Thecodont type, and are related to them generically, the beds containing them must be parallel to those beds in Europe which contain the similar types of organization; otherwise, the fossils of a distant country furnish us with no safe guides for our researches. Another important fact is the total absence of all the Mollusca which belong to the Jurassic series in Europe, which, when it is considered, is true of a series seven or eight thousand feet thick, should not fail to impress the minds of geologists, that this negative fact militates strongly against the idea that our sandstones of Connecticut, Virginia, and North Carolina are of that age. Then, again, we should not lose sight of the fact, that the physical features of the sandstones of both continents have many things in common. The irregularities in thickness, the frequent repetitions of conglomerates and breccias, mark a period when aspects must have been quite similar. Towards the close of this period in the upper sandstone, bones of a bird have been found, and single Saurian bones, accompanied with a few fish scales. There is, however, a great barrenness, thus far, of fossils in the upper beds. You have no reason to expect that they will hereafter prove richer, since the upper part has been cut through by the North Carolina Central Railway, without exposing any new beds of fossils which were before unknown.

The question of the Permianage of the bituminous slates and lower sandstone must turn upon the age of the Bristol beds, England, which have been referred to. Some of the most distinguished European geologists now refer them to the Bunter period; and there are certain facts disclosed by the beds of North Carolina which favor this view. An examination and comparison of the Saurians, however, of the Keuper, Muschelkalk, and Bunter sandstones with those of Deep and Dan rivers prove the absence of the Nothosaurus and other Enaliosaurs of the Trias. It is true, the teeth of the Rutiodon resemble the former, but the jaw is quite different, and the Rutiodon is clearly not an Enaliosaur. While we may admit that the age of the lower beds of the sandstones under consideration is not fully established, yet it seems we are borne out in the conclusion that they are infra-Liassic, and furthermore, that the upper masses of North Carolina, at least, are nearly or quite parallel with the Keuper of Europe.

III. ZOÖLOGY.

1. On the Structure of the Head in Vertebrata, and its Relations to the Phyllotactic Laws. By Theodore C. Hilgard, M. D., St. Louis, Mo.

THE MATHEMATICAL LAW OF PHYLLOTAXIS.

This well-known and remarkable law, established by Carl Schimper, in 1830, is, *in nuce*, the following: the NUMBER of leaves, scales, floral parts, etc., invariably forming distinct *sets*, whether by *singly* alternating around the stem, or as *whorls*, is always — with few exceptions, and which are secondarily thence derivable — one of these:

1, 2, 3, 5, 8, 13, 21, 34, 55, 89, 144, 233, 377, and so on — constituting a numeric series, which commences with the unit and progresses by the sums of each two ultimate members obtained. If placed at different heights, they regularly alternate in their respective radial positions in such a manner, that *immediately before an* (ichnographic) *repetition obtains, their sum amounts to one of the serial numbers*, and each two organs proximate in height, or along the stem, *throughout the set, diverge in the same direction of spiral*, and, by that number of ichnographic interstices, which, in the numeric series above given, is the *antepenultimate to the number of leaves comprised in the set*, or "cycle." Each new cycle commences with the element which repeats the position of the first member in the previous set, which, when the cycles repeat, may be assumed at any point, but, if otherwise, is to be counted from the element where divergence is different above and below, or the spiral assumes an opposite direction, as is sometimes the case between the various cycles of floral elements constituting the flower.

The number of elements, contained in a cycle of spiral disposition, divides the whole circumference into as many ichnographic interstices, each leaf, however, being at a different height. If designated by the ordinals as they *ascend* along the stem, if e. g. eight in number, their angular or *peripheric juxtaposition* will be the following, in the (*short-way*) direction of the spiral: I, IV, VII, II, V, VIII, III, VI, I, leaping three interstices, or two leaves, to arrive at the ordinal next in number; if five, they ichnographically succeed (in the short direction) thus: first, fourth, second, fifth, third, first, or lowest. This is called the law of DIVERGENCE.

My explanation of the *production* of these numbers and divergences, or *organic* law of Phyllotaxis, so kindly noticed by Prof. Peirce and Agassiz, at the last meeting and elsewhere, and more lengthily treated in the Proceedings of the St. Louis Academy of Science, is shortly this:—

Each foliar part, or phyllotactic element, originates, in the bud, of a single cell, at the termination of the succulent marrow-core, from amidst the others, in their very centre, and at their base. Cytogenesis, or origination of cells, always takes place out of a preëxistent one, according to modern researches. Hence any new element must spring up from within a cell, of either the marrow-core or an element already formed.

If the first element be supposed to produce the second, and the third between them, and that, at corresponding rates of maturity, each member commences and continues rhythmically to put forth single progeny, desisting by sets of coevals by rhythm, or, corresponding to the first element, then all the NUMBERS *of the series — and no others — are successively produced*, until checked by the effunction of the first cell, and, *corresponding to the same*, that of all the later ones; e. g., the fifth parturition of the first cell corresponding to the second one of the third member, and to the first one of Nos. IV and V. Each one later in degree and number is later in fulfilling its term, as the rudera of leaves spring up successively, and *not simultaneously* by sets of coevals. No. I, if effunct at the 5th term, and Nos. IV and V, correspondingly, at their first, then a cycle of 13 foliar elements is obtained.

If every issue be deposited on the side towards the ELDEST *of immediate neighbors, then the lawful* DIVERGENCE, *or, interpolation of new parts between the old ones*, (in their centre, originally, but assuming such radial direction,) *is effected.*

This suggested mode of *production* of the phyllotactic numbers and divergences, we would call the "GYRAL LAW," being a system of *elementary generation in ascension around the centre*, and *by distinct genealogies; a spiral progress of generation*, confirmatory of the view, that the progress both of *physical and dynamic light* — of *power and* *life*—ensues *in a spiral motion*; a view propounded by the original founder of the "atomic theory," the learned E. Swedenborg.

Torsion of Fibre.

My theory of the successive production and interpolation of subsequent elements at once affords a clue to the universal phenomenon of a natural "*warp*" or *torsion of fibre* in stems. Climbers twine in a direction *opposite to that of their torsion*, and to the latter the climbing itself is generally ascribed. If justly so, an actual torsion or twisting *force* would be required.

The *infra-foliar* respective *sectors of stems, developing under the influence of leaves*, in climbers are soft and slender; hence, the new elements, if *actually interposed*—as my theory requires—between more ancient and rigid parts, by their own *increase, while sprouting*, would be *distorted towards the free side*, which is in the direction of the spiral, while their base, far beneath, remains comparatively fixed, so as to produce a *continuous warp*, and that in the direction of the *phyllotactic spiral*; not identical with it, but having the same cause, and being considerably less in its amount, which is superadded to that of the foliar spiral.

If the torsion of fibre be actually derived from this process of origination, then the *coil* of the climber must be *opposite to the foliar spiral*, the latter requiring to be coincident with that of the fibre; and, as far as my observation goes, this opposition of coil and foliar spiral seems to obtain.

From the above, it would follow that there must be no coiling climbers with *whorls immediately corresponding*, or *crosswise either*; a *spiral alternation* of whorls being required, in verticillate stems, to produce a torsion.

The *rotary motion* of the drooping flagelliform *tops of vines*, abundantly established by observation, at once finds its explanation in this confirmed supposition of an actual *process of torsion*. When a resistance is met with, and the tip does not slip by, but is checked, then, probably by the very power which forces the top of the limb round its longitudinal axis, the *coiling* commences.

If such a limb be artificially coiled round a pole in a direction opposite to the one *each species of climber invariably maintains*, it *resists* that coil, and, if loosed after a while, *whirls round in the opposite direction*, no doubt by its very process of development, as above assumed; and if artificially kept in a wrong coil, its progress of growth is perceptibly *checked;* all of which is confirmatory of the theory, that it is the *crescive expansion which causes torsion.*

The *Direction of Spiral* is known to be one and the same in the same species of climbers.

In branched plants, trees, etc., the spiral direction is different in different branches, but said to average about equal odds in the whole tree.

The first *material* sign of the determination of spiral direction being the *position assumed by the third element*, — a late comer, and recondite within the most intrinsic and vital parts; it is likewise evident that *the first cause of that direction must be predetermined within the* BIO-DYNAMIC CONSTITUTION, and not caused by conditions extraneous to the vital peculiarity of the vegetable, the result being invariably the same, under whatever physical conditions placed, if not, as a factor, complicated with the vital phenomena themselves, to which, hence, the actuating cause requires to be referred.

CONCLUSION. — TRANSMITTED PROLIFICACY.

The very *termination of cycles by the serial numbers*, if produced by the cytogenetic process assumed, is demonstrative of a strict relation of every later issue of rudiments to the activity of the *first one*, and, likewise, of a coördinate equality of prolific power among the members of *equal grade* (although not time) *of descent*; the same number of parturitions being reserved to each member of the same grade, although brought to action gradually and *successively*, not by a simultaneous act. From this *solidarity of cycles*, and prolificacy *graduated and diminished in direct proportion to genealogical degree*, it would appear that the prolificacy of subsequent elements was *derived from the preceding ones*, by a sort of continuous "*seminal process without fructification*," (*vide* "Weisungen ueber die mosaische Schoepfungsgeschichte," p. 436, Wien, bei M. Auer, 1855,) a sort of "pullulation" of cells, upholding the vital processes and *change of substance* or actual *regeneration* of the living body itself, by cytogenesis, — and as a production of new *individuals* known in the genus Aphis, in the "change of generations," and in so many well-authenticated cases of botanical "parthenogenesis," as a *generation without material sexuality.*

A *first cell*, a *graduated prolificacy*, and a *cessation of prolification*, require a recommencement, by a new *cell progenitorial* of a future cycle, assuming in its first element a *certain position* toward that of the previous cycle; with *leaves*, a *corresponding* radial position; with *flowers*, mostly an *alternate* one, frequently combined with a change in the direction of spirals, but not among organs of equal function, as, e. g. repeated cycles of *petals or stamina.*

Thus, by its number and disposition, the *cycle* appears demonstrable as a thing *objectively constituted.*

ENCEPHALIC CYCLE.

The brain, embryology shows, originates in a *circle of three little vesicles*, to which, subsequently, two more are added, and *interpolated* in a *gyral sense;* the 4th, between the first and second; the 5th, between the second and third. The first vesicle subsequently develops into the "*medulla oblongata*," with the "*corpora olivaria*" as a pair of ganglia or "*nuclei*," and gives rise to all the tactile and motor nerves of the sensuous organs.

The second vesicle develops into the "*corpora quadrigemina*," replete with "nucleolar" (*grey*) substance, and, together with the *gustative* pair of nerves, *arises from the first element.*

The third vesicle develops into the "*corpora striata*," and gives rise to *olfactory* nerves.

The fourth section of the brain, the "*cerebellum*" with the "pons Varolii," arising between the first and second element, passes the *auditory* nerves. It contains a pair of "*nuclei cerebelli.*"

The fifth section, arising between the second and third, develops into the major part of the brain, with the "*thalami optici*," as ganglia, giving rise to the *optic* nerve.

The cerebral *hood* or circle, afterwards by development, stretches out into a straight line, perhaps as the foliar elements *arise along a scion*, or, by a *rupture of the cycle* between the first and third elements.

The segments of the brain seem to *originate* in the ascending succession, in which their corresponding senses are, *by common agreement*, placed: 1st, taction; 2d, gustation proper; 3d, olfaction; 4th, audition;

5th, vision. But the *local* succession of themselves and their *contingent cerebral vertebræ*, is that of gyral *ordinals along the periphery:* 1st, 4th, 2d, 5th, 3d, — taction, audition, gustation, vision, olfaction, — like a ring gyrally developed as a quinary cycle, *ruptured between the first and third element*, and somewhat stretched, much in the fashion of the apex of a *crosier*.

A *simile* of this obtains in the successive lengths of the five fingers; if the thumb be accounted the second in magnitude, they follow thus: middle-finger, thumb, gold-finger, index, little finger, — like the calyx of a rose-bud if *ruptured between the fifth* (smallest) *and second* element.

STRUCTURE OF THE HEAD IN VERTEBRATA.

Interpretation of the Skull.

Oken and Carus were the first to explain the segments, forming the cranial cavity, as *homologues to vertebræ*.

Each vertebra consists of five essential parts: one vertebral *block* ("body"); two *flank*-pieces ("neurapophyses"), with an incision or perforation transmitting a proper pair of nerves; and two *arch or top*-pieces ("neural spine"). These parts originate singly, and meet by sutures; and in the spine, mostly by a substantial coalescence, subsequently unite where they meet.

There are also a pair of "*transverse processes*" ("pleurapophyses") at the base of the arch. On the cranium they appear as mere *ridges*, apparently proper to the vertebral *bodies*, and by pairs *corresponding to their original number*.

Oken recognized four vertebræ or neural arches. The first, he considered the occiput, with the *basilar bone* or "*clivus Blumenbachii*," for a block, with the "*pars petrosa*" of the temporal bone, on either side, for its *flank*-pieces, perforated by the auditory nerve, and with the tabulate part as its *vault* or "neural spine."

The second, according to him, was constituted by the "*sphenoid*" bone and parietal bones, the former consisting of a *block* and two "*alæ magnæ*," with a posterior incision for the passage of the glosso-pharyngeal nerve. Besides, the "*processi ensiformes*" on either side, as *flank*-pieces of the "visual" vertebra, give passage to the optic nerve; the *arch*-pieces to which are the frontal bones; and the vertebral *body*, distinct in animals, is contiguous to that of the sphenoid bone, and, in man, much reduced and coalesced with it.

This was the third ring. As the fourth, he considered the ethmoidal bone, with the *crista galli* and *lamina perpendicularis* for a body, the perforated sides as flank-pieces, and the nasal bones as arch-pieces; the latter, however, receding from the cranial cavity.

Carus adopted six vertebral rings,—three to form the *cranium*, three the *face*,—basing on the tabulate facial bones of *Myxine*, a cartilaginous fish. In all others, however, the facial bones are developed after the type of *extremities*, which likewise, in an embryonic state, first appear as *slabs*.

According to Oken, each of the cranial vertebræ gives passage to one *pair of specific nerves*; claiming, on anatomical grounds, the glossopharyngeal nerve as that of gustation,—the real function of the *lingual sense* then not being known, and its nerve remaining in litigation.

As I have demonstrated at a former meeting (Proceedings of 1854), the *specific* lingual sense, residing in the surface of the *tongue* and *soft palate*, comprises merely *sour*, *sweet*, *salt*, *bitter*,—all the rest being olfactions. When the glossopharyngeal nerve has been severed, animals no longer reject bitter food. Bitterness being tasted *all over* the tongue and soft palate, it was concluded by Mueller, that the trigeminate nerve ("nervi palatini") was concerned in this specific gustation, and all followed his accidental mistake. But the glossopharyngeal nerve supplies not only the surface of the tongue with its *sensitive* fibres, but *also the soft palate*, by numerous branches from its *circulus tonsillaris*,—and *no other parts*. Thus the nerve, I claim, covers the *whole* area, and does *not extend beyond*,—which I beg to correct in my previous essay.

Actual Number of Cranial Vertebræ.

A comparison with the five specific cycles of the *flower*—of which hereafter—seemed to require a *fifth*, or rather another *first* vertebra of the skull, and that must be subservient to the *cutaneo-motor* system, or the sense of *touch* and fleshy fabric.

The proposition once conceived, I found the following proofs in confirmation.

First and Second Vertebra.

The posterior half of the basilar bone is marked with a pair of ridges, or *transverse processes*, corresponding to an anterior one, and by their presence, demonstrative of *a proper vertebral body*, of which the flank-pieces are the condyloid processes, perforated by the *motile* hypoglossic nerve, as a homologue to the *os petrosum* of the auricular vertebra, which I hold to be the flank-piece of the *second* in succession.

The *arch*-pieces of the *first* vertebra, I first discovered in a withered lamb's skull, with a *cross suture across the occiput*, thus divided into two sets of top-pieces; the first set, *proceeding from the condyle*, with a *median suture* in the longitudinal dividing line of the cranium; while the rhombic anterior part of the occipital bone, severed from it, remained as an apparently single piece, its median suture being fused, as in the insequent parietal bones of the sheep.

The suture dividing the arch-pieces of the first, or "motile," from those of the second, or "auditory," vertebra, obtains, perhaps, in *all mammals*, the first being generally more strongly developed, and serving for the insertion of the powerful neck-muscles, (compare cat and musk-rat,) while the actual vault of the auditory vertebra, under stress of the temporal bones, in the cat, is reduced to the appearance of a small triangular intercalary osselet; still being the strict homologue of the rhombic vault-piece, or "key-stone," (Owen,) in the cranium of fishes, mammals, etc.

Infant's Skull.

All these homologous parts and their arrangement, I found very plainly expressed in the infant European skull, viewed from the inside. The flight of *five* pairs of perforated flank-pieces, like the ribs of a ship, and in homology to the *bicapitate ribs* of the "hæmal arch," is very evident. The occipital *squama* is *almost cross-divided* by two fissures, and Tschudi informs us, and figures, that even the adult *skull of ancient Peruvians* has a *transverse suture across the occiput*. It is *this* suture which divides the occiput of vertebrata, leaving the "keystone" to represent the fused arch-pieces of the second vertebra.

Temporal Bones.

Apart of its perforated *pars petrosa* being the *neural rib*, or "neurapophysis," to the auditory vertebra, the temporal bone typically consists of *three separate elements*, generally forming a *glenoid cavity* for the reception of the lower jaws. In the fish skull, it is inserted into the large crevices between the vertebral elements proper, thus forming part of the cranial vault; in the sheep, cat, and infant skull, it can be cleft away without considerably opening the cranial cavity. The temporal bones invariably are the *tripartite coxal apparatus* of the lower jaw, or *extremity belonging to the lingual vertebra.*

Nasal Vertebra.

The skull of *Catostomus* (buffalo-fish of the Mississippi) has this vertebra largely developed in a long block-piece, a pair of perforated flank-pieces, and a fused tabulate top-piece. This top-piece, by a downward crest, *forms the partition between the ethmoidal bones*, and by a suture is *severed from the block* of this vertebra. It remains the true homologue of the "*crista galli*" and "*lamina perpendicularis.*" In amphibia, birds, and mammals, its top-pieces *remain fused*, not distinct, as the nasal bones are, and are gradually *overlapped by the frontal bone* (birds), so as to be *withdrawn from the surface*, forming an actual part of the cranial cavity, in the shape of the *median ridge of the ethmoidal bone*—the *block* of the nasal vertebra being very much shortened, and found between the *processi ensiformes* of the sphenoid bone and the commencement of the *lamina cribrosa* of the ethmoidal bone.

Fish-Head.

My (hitherto hypothetical) "*first vertebra*" is developed largely and *independently* in all its parts in the skull of the cat-fish (Pimelodus), whilst *preserving all the homologies to the posterior part of the occiput* of other fishes and vertebrata in general.

In the fish, the skull, at its flat base, destitute of palatal processes, harbors a cylindric *cephalothorax* of *five* pair of ribs, bearing the fringes of the gills, and in number corresponding to the *five vertebræ* of the skull, as here assumed. These ribs are loosely attached to the base of the skull at their upper end, and below meet in a *sternum*.

Besides these ribs, *each cranial vertebra*, except apparently the "first" one, *bears one pair of appendages*, each being a perfect homologue to the *pairs of extremities* of the trunk.

Oken demonstrated the osseous ring behind the gill-slit to be the true homologue of the *upper extremities*, inasmuch as in higher animals it *gradually recedes*, forming itself the *shoulders and arms*. In the fish, it arises from the *auricular* vertebra's flank-pieces.

Nasal Extremities.

In *Catostomus* the nasal vertebra bears a finely developed pair of *movable extremities*, by which the snout is porrected. In other fishes, and in amphibia, it is seen to gradually merge into the *nasal bones* and intermaxillaries.

Visual Extremities.

The skull of the *cock* shows, at the anterior end of the large orbit, one tripartite, coxal apparatus, which belongs to the intermaxillaries, and another, the temporal coxa, at its posterior end. Besides, from *within* the orbit emerges a striking analogue of the bird shoulder and wing (arm): a stout *clavicle*, at its top bearing an *uncinate process*, and a slender *zygomatic blade*, giving rise, at their point of junction, to a palatal homologue of a *humerus*, its *elbow* touching that of the other side, and issuing into a palatal fore-arm. The similarity is so striking, that, if detached, it might be passed for the shoulder and wing of a large humming-bird.

In the fish, this pair of extremities, much resembling in its structure the *digitate antennæ* of insects, is removed downwards, its coxal apparatus forming a sort of *manubrium sterni* to the cephalothorax, as, in man, the upper extremities, by their clavicles, join the *sternum* and thoracal fabric. This visual extremity of fishes is porrected backward like the legs and feet of the seal, and its numerous digitate *rays* are applied to the lower jaw, forming part of the lid of the gill-slit. In poisonous snakes, these rays form the *erectile fangs*, movable on a sort of foreshortened hand and *arm*. In mammals, this structure retreats toward the vertebral blocks, its clavicle, humerus, and uncinate process forming, on either side, the *pterygoid process of the sphenoid bone.*

In birds, the lower jaw, being *dislodged from the temporal bones* — its proper coxa — also *hinges on the pterygoid coxa,* together with the palatal extremity properly thereunto belonging, the lower jaw being *suspended from the temporal bones* merely by muscles and sinews.

In the fish, we find the large lid-bone of the gill-slit apparently belonging to a pair of extremities, perhaps as a *phalangal* bone, and perhaps of the eye extremity. In *Ephippium,* as remarked by zoölogists heretofore, this plate is seen to gradually be *converted into the tympanic bone,* and *tympanum* itself.

The Gustative Extremities

are the temporal bones, as a coxal tripod, and the lower jaw as its members.

The Auricular Extremities

are the osseous ring, dividing the head from the body, and situated directly behind the gill-slit. They, originally, in fish, arise from the auricular vertebra, to which they are firmly attached, and in higher animals gradually assume the development and position of the *upper extremities,* by *displacement from their respective vertebra.*

In the cat-fish (Pimelodus), the uncinate process of the auricular coxa, or *shoulder,* forms the stout ray of the pectoral fin. The pectoral *plate,* strongly indented (*sutured*) with the opposite one and closing the ring, as the palatal humeri likewise approximate with their elbows, is the auricular or proper *humerus,* by analogy with *Catostomus,* etc., and is probably *the analogue of the so-called lower jaw of insects.*

Lower Extremities.

Another pair of extremities — the "lower," or *pelvic* one — remains to be *referred to its originating vertebra,* while the first cranial vertebra, so conspicuously developed in *Pimelodus,* from analogy seems to require a *corresponding pair of extremities.* No doubt the pelvic fins, hind legs, or lower extremities, are the *ones corresponding to the first* *cranial vertebra*, but are removed like the visual one in the fish, and the auricular one, or arms, in higher animals.

In the human fabric—the last and highest of vertebral types known—the whole of the vertebral system is received into the appendages of the head, namely, between the nasal bones, the jaws and the extremities. Man is erected within his head, as it were.

Parallelism between Intellectual and Sensuous Conceptions.

There is a natural correspondence between the divisions of the brain and their specific nerves, as above demonstrated, on the one hand, and between the intellectual functions and sensuous functions on the other—"feelings" being compared to feeling; "tastes," or appreciation of the *congenial* and *pleasurable*, to gustation and olfaction; "ear" and "audition" to *comprehension*, the extremities, proper to its vertebra, the arms and hands being likewise the *prehensile* ones; *discrimination* to seeing. It is not improbable that a consistent theory of psychology and psychic organology may spring from the comparison between intellectual and sensuous processes and organs on the one hand, and organologic developments of flowers on the other, as I would indicate by the following outlines of a comparison between these four: the sensuous apparatus, the sexual organism, and the digestive system, on the one hand, and the *five cycles of perfect flowers*, on the other,—a parallelism first essayed by the late Oken.

The Anthic Law.

The perfect flower, such as the rose and apple, consists of *five cycles*, which *successively alternate in position*, except the last, the *embryonic* one, or seed, which is contained within the fourth or *utricular* one, *i. e.*, the capsule, alternate to whose lobes are the *antheric*, *glandular*, *or punctiferous* (celluliferous) organs, to which the calyx, first or *somatic* cycle, corresponds in position; while the second, or *ligulate* cycle, the corolla, alternating with the former, in its position, is coincident with both the utricular and the embryonic ones. The calyx and antheral lobes, forming one quinal star, alternate with the other, which comprises the petals, follicles, and seeds.

Want of space does not now permit me to give more than a mere statement of correlations, the stringency whereof I must leave for another occasion to develop, but give a short synopsis, in order to complete the schematic interpretation of the structure of the head.

In the animate body, the calyx is represented in the *cutaneous* or truncal lobes; the flower, in the *ligulate;* the antheric, in the *glandular;* the capsular, in the *utricular;* the embryonic, in the *disciferous*: the lenses, tympani and glottis, and the embryo.

The sexual organs are known to exactly correspond in the sexes after the following manner: the uterine segments and seminal vesicles are homologues; so are the ovaries and *testes*, the *corpora cavernosa clitoridis* and *do. penis*, and, to my views, the *labia interna*, as a single, erectile, ligulate or laminate element, to the *corp. cav. urethræ.*

Oken demonstrated a correspondence between the external earducts and the tubæ; the internal ear-ducts, (*tubæ Eustachianæ,*) and the womb-segments; the *parotides* and the ovaries and *testes;* and between the tongue and erectile laminæ. I commence with the utricles, as the centre, harboring, at their opening, like a matured *seed*, or *focus of life*, the discoid organs, and appendaged with the corresponding *laminate* or ligulate ones, as with their contingent petals, between which enter the *cutaneous lobes* of embryology, enclosing each a corresponding *gland* or *antheral* organ.

In the head we have the two *choanæ and lachrymal canals* as the first *pair of utricles*, harboring, at their extremities, the two *lenticular systems* of the eyes, *wrapt* into the laminal appendages, the *eye-chalyces* of the embryonic state, which afterwards close up, by a suture, to eyeballs. The second pair, likewise meeting at the fauces, are the *internal ear-ducts*, with *tympani* as discoid organs, and *external ducts and conchs* as ligulate appendages. The *odd* utricle is the *fauces*, originally distinct (by *atresia*) from the intestines. The *ligulate appendage* of the faucal utricle is the *tongue;* its *disc*, the *glottis*. Their respective *stigmas* are the epiglottis, the ear-valves, and the lachrymal caruncles.

The *odd* cutaneous lobe, in the embryonic state, is the forehead, nose, and central piece of the upper lip. As its gland, it contains the osseous intricacies of the nose, and, chiefly, the *lungs*. The insequent *pair of* *lobes* are the *cheeks*, containing, as their glands, the parotides. In the angle between the cheeks and forehead, the eyes are located by the above law of *alternation of cycles.*

The *second pair* of cutaneous lobes are those which ultimately unite at the chin, and probably (*musculus platysma myoides*) comprise the *mammal* lobes and their respective glands. In the angle between them and the cheeks the external ear obtains, and *between* them the tongue is *included.*

The odd sexual *cutaneous lobe* proceeds from the umbilicar region, and forms the prepuce, with a *frenulum* indicative of a "ventral suture" of margins. To it correspond the *kidneys* as *antheraic and gland,* with the lungs as their paragon. In the angle between it and the *labia externa*, or scrotal lobe, end the *ligamenta uteri teretia*, of an originally utricular character, and terminated by the erectile laminæ, called *corpora cavernosa clitoridis*, homologues to the *corp. cav. penis*, in the males. The first pair of cutaneous lobes include the *testes*, whose homologues are the ovaries. The second pair of lobes are the *nates*, containing the *glandular Cowperi s. Berthelinianæ.* Between them and the scrotal lobes no doubt belong, schematically, the *tubæ*, with the segments of the *womb as utricular elements.* They are, however, retracted into the intestinal cavity, and correspond to the ears no less by this recondite position. Between the nates, in the suture, appertains the *corp. cav. urethræ* in the male, the doubled *labian internum* — a simile to the odd labellum of orchids — of the female. The odd of the five utricular organs is the *vagina* and its correspondent in the male, with the tripartite *hymen* as *discoid element* (compare *tympanum*), and giving rise to the odd one of erectile laminæ.

We return to the glands. Besides the glandular cycle above mentioned, a second glandular or antheral one obtains in the facial, as in the sexual "flower." This second cycle is, of course, coincident with the laminal appendages, the *thyreoid gland* with the tongue, the *tonsils* with the ears, or, in the embryonic state, gill-slits, the *lachrymal glands* with the eyes.

The cutaneous nerves pervade the *skin* and fabric generally, being subservient to localization. The gustative nerve expands on the *lamina*, the tongue; the olfactory one, in the osseous *sinuosities* (glandular?); the auditory, on the continuation of the *utricle* into the "*os pe-*

trosum" ; the *optic*, on the surface of the transparent system of refraction.

Thus it appears that each of the specific sensations, *by its organs* and sexual homologues, as well as by their intellectual correspondents, has a specific relation to the *cycles* of the flower, to its *functions and form generally*. Therefore the idea may not be void of plausibility that the *special* forms of flowers, or combination of their elements, may also correspond to the *subdivisions* of our sensations and intellectual processes, and their combination into a whole to intellectual combinations, as hinted above.

The five floral cycles correspond to the five nutritive systems, which take their rise, embryonically, from the navel. Reproduction or "regeneration," *development* and *change of substance*, may be said to be essentially a process of *generation*, likewise, as hinted above.

From a simple stretched vascular duct, the five-valved and four-chambered *heart* is developed; from a short and straight intestine, the tortuous *bowels*, with five chief sections, *œsophagus*, *stomach*, *duodenum ileum*, *colon*. To the vascular system belongs probably, as an *independent fifth utricle*, or chamber, the *vena portarum*. Two *glands Thymus*, *pancreas* and *glands suprarenales* are a set, and the spleen and lymphatic glands another one, while the fauces and lungs above the intestine, and the bladder, liver, and kidneys below it, belong partly to the sensuous, partly to the sexual system.

A minute study of the gradual development of the intestinal organs would no doubt serve to throw more light on their systematic construction. From a comparison with the corresponding organs of the head and pelvis, the *skeleto-kreatine* part of the trunk may correspond to the cutaneous system, whose function is *localization ;* the *mucous* ducts, by a comparison with the *vagina* and *urethra*, as surfaces of ligulate organs, to the second, gustative, or *assimilatively testing* cycle ; the lobed liver, spleen, and pancreas to the *glandular or vitalizing* cycle ; the lymphatic and *vascular utricles*, centering in the *heart and portal vein*, to the fourth or *vibrating and conducting* cycle ; and that ocean of *vitalized discoid lenticles*, the blood, to the fifth or *fociferous* cycle ; the systems and organs of *regeneration* affording an evident analogy to those of *generation* and the *anthic fabric* generally.

IV. ETHNOLOGY.

1. Thoughts on Species. By James D. Dana, of New Haven, Conn.

While direct investigation of individual objects in nature is the true method of ascertaining the laws and limits of species, we have another source of suggestion and authority in the comprehensive principles that pervade the universe. The source of doubt in this synthetic mode of reaching truth, consists in our imperfect appreciation of universal law. But science has already searched deeply enough into the different departments of nature, to harmonize many of the thoughts that are coming in from her wide limits; and it is well, as we go on in research, to compare the results of observations with these utterings of her universality.

I propose to present some thoughts on species from the latter point of view, reasoning from central principles to the circumferential, and, if I mistake not, we shall find the light from this direction sufficiently clear to illumine a subject which is yet involved in doubts and difficulties.

The questions before us at this time are, —

1. What is a species?
2. Are species permanent?
3. What is the basis of variations in species?

1. *What is a species?*

It is common to define a species as *a group* comprising such individuals as are alike in *fundamental* qualities; and then, by way of elucidation, to explain what is meant by fundamental qualities. But the idea of a group is not essential; and moreover it tends to confuse the mind by bringing before it, in the outset, the endless diversities in individuals, and suggesting numberless questions that vary in answer for each kingdom, class, or subordinate group. It is better to approach the subject from a profounder point of view, search for the true idea of distinction among species, and then proceed onward to a consideration of the systems of variables.

Let us look first to *inorganic* nature. From the study of the inorganic world, we learn that each element is represented by a specific amount or law of force; and we even set down in numbers the precise value of this force as regards one of the deepest of its qualities, chemical attraction. Taking the lightest element as a unit to measure others by, as to their weights in combination, oxygen stands in our books as 8; and it is precisely of this numerical value in its compounds: each molecule is an 8 in its chemical force or law, or some simple multiple of it. In the same way, there is a specific number at the basis of other qualities. Whenever, then, the oxygen amount and kind of force was concentred in a molecule, in the act of creation, the species oxygen commenced to exist. And the making of many such molecules instead of one, was only a repetition in each molecule, of the idea of oxygen.

In combinations of the elements, as of oxygen and hydrogen, the resultant molecule is still equivalent to a fixed amount, condition, or law of chemical force; and this law, which we express in numbers, is at the basis of our notion of the new species.

It is not necessarily a different amount of force; for it may be simply a different state of concentration, or different rate or law of action. This should be kept in mind in connection with what follows.*

The essential idea of a species, thence deduced, is this: a *species* corresponds to *a specific amount or condition of concentred force, defined in the act or law of creation.*

Turn now to the organic world. The individual is involved in the germ-cell from which it proceeds. That cell possesses certain inherent qualities or powers, bearing a definite relation to external nature, so that, when having its appropriate nidus or surrounding conditions, it

* When we have in view oxygen and the elements, we are apt to think of their molecules as distinguished by a different *amount* and *kind* of force. But when we consider the many different compounds that may be made of the same elements (as carbon and hydrogen), in the very *same* proportions, we are led to conceive of these as differing molecularly in a different *law* of the same force or forces. When, again, we see the same element under conditions as diverse as any two compounds, as in cases of allotropism, we are still better satisfied with adopting, for the present, the most general expression — a different law of action or condition of molecular force.

will grow, and develop out each organ and member to the completed result, and this, both as to all chemical changes, and the evolution of the structure which belongs to it as a subordinate to some kingdom, class, order, genus, and species in nature. The germ-cell of an organic being develops a specific result; and like the molecule of oxygen, it must correspond to a measured quota or specific law of force. We cannot apply the measure, as in the inorganic kingdom, for we have learned no method or unit of comparison. But it must nevertheless be true, that a specific predetermined amount, or condition, or law of force, is an equivalent of every germ-cell in the kingdoms of life. I do not mean to say that there is but one kind of force; but that whatever the kind or kinds, it has a numerical value or law, although human arithmetic may never give it expression.

A species among living beings, then, as well as inorganic, is based on *a specific amount or condition of concentred force defined in the act or law of creation.*

Any one species has its specific value or law of force; another, its value; and so for all: and we perceive the fundamental notion of the distinction between species, when we view them from this potential stand-point. The species, in any particular case, began its existence when the first germ-cell or individual was created; and if several germ-cells of equivalent force were created, or several individuals, each was but a repetition of the other: the species is in the potential nature of the individual, whether one or many individuals exist.

Now in organic beings — unlike the inorganic — there is a cycle of progress involving growth and decline. The oxygen molecule may be eternal as far as any thing in its nature goes. But the germ-cell is but an incipient state in a cycle of changes, and is not the same for two successive instants; and this cycle is such that it includes in its flow, a reproduction, after an interval, of a precise equivalent of the parent germ-cell. Thus an indefinite perpetuation of the germ-cell is in fact effected; yet it is not mere endless being, but like evolving like in an unlimited round. Hence, when individuals multiply from generation to generation, it is but a repetition of the primordial type-idea; and the true notion of the species is not in the resulting group, but in the idea or potential element which is at the basis of every individual of the group; that is, the specific law of force, alike in all, upon which the power of each as an existence and agent in nature depends. Dr.

Morton presented nearly the same idea when he described a species as *a primordial organic form.*

Having reached this idea as the starting point in our notion of a species, we must still, in order to complete and perfect our view, consider what is the true expression of this potentiality. For this purpose, we should have again in mind, that a living cell, unlike an inorganic molecule, has only a historical existence. The species is not the adult resultant of growth, nor the initial germ-cell, nor its condition at any other point; it comprises the whole history of the development. Each species has its own special mode of development as well as ultimate form or result, its serial unfolding, inworking, and outflowing; so that the precise nature of the potentiality in each is expressed by the line of historical progress from the germ to the full expansion of its powers, and the realization of the end of its being. We comprehend the type-idea only when we understand the cycle of evolution through all its laws of progress, both as regards the living structure under development within, and its successive relations to the external world.

2. *Permanence of species.*

What now may we infer with regard to the permanence or fixedness of species from a general survey of nature?

Let us turn again to the inorganic world. Do we there find oxygen blending by indefinite shadings with hydrogen or with any other element? Is its combining number, its potential equivalent, a varying number, — usually 8, but at times 8 and a fraction, 9, and so on? Far from this, the number is as fixed as the universe. There are no indefinite blendings of elements. There are combinations by multiples or submultiples, but these prove the dominance and fixedness of the combining numbers.

But further than this, fixed numbers, definite in value, and defiant of all destroying powers, are well known to characterize nature from its basement to its top-stone. We find them in combinations by volume as well as weight, that is, in all the relations of chemical attraction; in the mathematical forms of crystals and the simple ratios in their modifications, — evidence of a numerical basis to cohesive attraction; in the laws of light, heat, and sound. Indeed, the whole constitution of inorganic nature, and of our minds with reference to nature, as Professor Peirce has well illustrated, involves fixed numbers; and the universe is not only based on mathematics, but on finite determinate numbers in the very natures of all its elemental forces. Thus the temple of nature is made, we may say, of hewn and measured stones, so that, although reaching to the heavens, we may measure, and thus use the finite to rise toward the infinite.

This being true for inorganic nature, it is necessarily the law for all nature, for the ideas that pervade the universe are not ideas of contrariety, but of unity and universality beneath and through diversity.

The units of the inorganic world are the weighed elements and their definite compounds, or their molecules. The units of the organic are *species*, which exhibit themselves in their simplest condition in the germ-cell state. The kingdoms of life, in all their magnificent proportions, are made from these units. Were these units capable of blending with one another indefinitely, they would no longer be units, and species could not be recognized. The system of life would be a maze of complexities; and whatever its grandeur to a being that could comprehend the infinite, it would be unintelligible chaos to man. The very beauties that might charm the soul would tend to engender hopeless despair in the thoughtful mind, instead of supplying his aspirations with eternal and ever-expanding truth. It would be to man the temple of nature fused over its whole surface and through its structure, without a line the mind could measure or comprehend.

Looking to facts in nature, we see accordingly everywhere, that the purity of species has been guarded with great precision. It strikes us naturally with wonder, that even in senseless plants, without the emotional repugnance of instinct, and with reproductive organs that are all outside, the free winds being often the means of transmission, there should be rigid law sustained against intermixture. The supposed cases of perpetuated fertile hybridity are so exceedingly few as almost to condemn themselves as no true examples of an abnormity so abhorrent to the system. They violate a principle so essential to the integrity of the plant-kingdom, and so opposed to nature's whole plan, that we rightly demand long and careful study before admitting the exceptions.

A few words will explain what is meant by perpetuated fertile hybridity. The following are the supposable grades of results from intermixture between two species: —

1. No issue whatever — the usual case in nature.

2. Mules (naming thus the issue) that are wholly infertile whether among themselves or in case of connection with the pure or original stock.

3. Mules that are wholly infertile among themselves, but may have issue for a generation or two by connection with one of the original stock.

4. Mules that are wholly infertile among themselves, but may have issue through indefinite generations by connection for each with an individual of the original stock.

5. Mules that are fertile among themselves through one or two generations.

6. Mules that are fertile among themselves through many generations.

7. Mules that are fertile among themselves through an indefinite number of generations.

The cases 1 to 5 are known to be established facts in nature; and each bears its testimony to the grand law of purity and permanence. The examples under the heads 2 to 5 become severally less and less numerous, and art must generally use an unnatural play of forces or arrangements to bring them about.

Again, in the animal kingdom, there is the same aversion in nature to intermixture, and it is emotional as well as physical. The supposed cases of fertile hybridity are fewer than among plants.

Moreover, in both kingdoms, if hybridity be begun, nature commences at once to purify herself as of an ulcer on the system. It is treated like a disease, and the energies of the species combine to throw it off. The short run of hybridity between the horse and the ass, species very closely related, reaching its end *in one single generation*, instead of favoring the idea that perpetuated fertile hybridity is possible, is a speaking protest against a principle that would ruin the system if allowed free scope.

The finiteness of nature in all her proportions, and the necessity of finiteness and fixedness for the very existence of a kingdom of life, or of human science, its impress on finite mind, are hence strong arguments for the belief that hybridity cannot seriously trifle with the true units of nature, and at the best, can only make temporary variations.

It is fair to make the supposition, that, in case of a very close proximity of species, there might be a degree of fertile hybridity allowed;

and that a closer and closer affinity *might* give a longer and longer range of fertility. But the case just now alluded to seems to cut the hypothesis short: and, moreover, it is not reasonable to attribute such indefiniteness to nature's outlines, for it is at variance with the spirit of her system.

Were such a case demonstrated by well-established facts, it would necessarily be admitted; and I would add, that investigations directed to this point are the most important that modern science can undertake. But until proved by arguments better than those drawn from domesticated animals, we may plead the general principle against the *possibilities* on the other side. If there is a law to be discovered, it is a wide and comprehensive law, for such are all nature's principles. Nature will teach it not in one corner of her system only, but more or less in every part. We have, therefore, a right to ask for well-defined facts, taken from the study of successive generations of the interbreeding of species known to be distinct.

Least of all should we expect that a law, which is so rigid among plants and the lower animals, should have its main exceptions in the highest class of the animal kingdom, and its most extravagant violations in the genus Homo; for if there are more than one species of man, they have become in the main indefinite by intermixture. The very crown of the kingdom has been despoiled; for a kingdom in nature is perfect only as it retains all its original parts in their full symmetry, undefaced and unblurred. Man, by receiving a plastic body, in accordance with a law that species most capable of domestication should necessarily be most pliant, was fitted to take the whole earth as his dominion, and live under every zone. And surely it would have been a very clumsy method of accomplishing the same result, to have made him of many species, all admitting of indefinite or nearly indefinite hybridization, in direct opposition to a grand principle elsewhere recognized in the organic kingdoms. It would have been using a process that produces impotence or nothing among animals, for the perpetuation and progress of the human race.

There are other ways of accounting for the limited productiveness of the mulatto, without appealing to a distinction of species. There are causes, independent of mixture, which are making the Indian to melt away before the white man, the Sandwich Islander and all savage people to sink into the ground before the power and energy of higher intelligence. They disappear like plants beneath those of stronger root and growth, being depressed morally, intellectually, and physically, contaminated by new vices, tainted variously by foreign disease, and dwindled in all their hopes and aims and means of progress, through an overshadowing race.

We have therefore reason to believe, from man's fertile intermixture, that he is one in species; and that all organic species are divine appointments which cannot be obliterated, unless by annihilating the individuals representing the species.

It may be said, that different species in the inorganic world combine so as to form new units, and why may they not in the organic? It is true they combine, but not by indefinite blendings. There is a definite law of multiples, and this is the central idea in the system of inorganic nature. In organic nature, such a law of multiples, if existing, would be general, as in the inorganic; it would be an essential part of the system, and should be easily verified, while, in fact, observation lends it no support, not even enough to have suggested the hypothesis.

In one kingdom, the *inorganic*, there is multiplication of kinds of units by combination, according to the law of multiples, and no reproduction; while in the *organic*, there is reproduction of like from like, and no multiplication of kinds by combination. And thus the two departments of living and dead nature widely diverge.

Neither does the possibility of mere mixture among inorganic substances afford any analogy to sustain the idea of possible hybrid mixture indefinitely perpetuated, among living beings. The mechanical aggregation of units that make up ordinary mixture, is one thing; and the combination that would alter a germ, one of the units in organic species, even to its fundamental nature, is quite another. This last is not aggregation. It is as different from mere mixture as is chemical combination, and stands somewhat in the same relation, so that the analogy has no bearing on the question.

3. *Variations of Species.*

But there are variations in species, and this is our next topic. The principles already considered teach, as we believe, that each species has its specific value as a unit, which is essentially permanent or indestructible by any natural source of change; and we have, therefore, to admit in the outset, if these principles are true, that variations have their limits, and cannot extend to the obliteration of the fundamental characteristics of a species.

To understand these variations, we may again appeal to general truths.

Variation is a characteristic of all things finite, and is involved in the very conditions of existence. No substance or body can be wholly independent of every or any other body in the universe. The most comprehensive and influential law in nature, most fundamental in all change, composition or decomposition, growth or decay, is the law of mutual sympathy, or tendency to equilibrium in force through universal action and reaction.

The planets have their orbits modified by other bodies in space through their changing relations to those bodies. A substance, as oxygen or iron, varies in temperature and state of expansion from the presence of a body of different temperature; in chemical tendencies from the presence of a luminous body like the sun; in magnetic or electrical attraction from surrounding magnetic or electrical influences. There is thus unceasing flow and unceasing change through the universe. All the natural forces are closely related as if a common family or group, and are in constant mutual interplay.

The degree or kind of variation has its specific law for each element; and in this law the specific nature of the element is in a degree expressed. There is to each body or species, the normal or fundamental force in which its very nature consists; and, in addition, the relations of this force to other bodies, or kinds, amounts or conditions of force, upon which its variations depend. One great end of inorganic science is to study out the law of variables for each element or species. For this law is as much a part of an idea of the species, as the fundamental potentiality; indeed, the one is a measure of the other.

So again, a species in the *organic* kingdoms is subject to variations, and upon the same principle. Its very development depends on the appropriation of material around it, and on attending physical forces or conditions, all of which are variable through the whole of its history. Every chemical or molecular law in the universe is concerned in the growth, — the laws of heat, light, electricity, cohesion, etc.; and the progress of the developing germ, whatever its primal potentiality, is unavoidably subject to variations, from the diversified influences to which it may be exposed. The new germ, moreover, takes peculiarities from the parent, or from the circumstances to which its ancestry had been exposed during one or more preceding generations.

There is then a fixed normal condition or value, and around it librations take place. There is a central or intrinsic law, which prevents a species from being drawn off to its destruction by any external agency, while subject to greater or less variations under extrinsic forces.

Liability to variation is hence part of the law of a species; and we cannot be said to comprehend in any case the complete idea of the type until the relations to external forces are also known. The law of variables is as much an expression of the fundamental qualities of the species in organic as in inorganic nature; and it should be the great aim of science to investigate it for every species. It is a source of knowledge which will yet give us a deep insight into the fundamental laws of life. Variations are not to be arranged under the head of *accidents*; for there is nothing accidental in nature; what we so call, are expressions really of profound law, and often betray truth and law which we should otherwise never suspect.

This process of variation is the external revealing the internal through their sympathetic relations: it is the law of universal nature reacting on the law of a special nature, and compelling the latter to exhibit its qualities: it is a centre of force manifesting its potentiality, not in its own inner working, but in its outgoings among the equilibrating forces around, and thus offering us, through the known and physical, some measure of the vital within the germ. It is therefore one of the richest sources of truth open to our search.

The limits of variation it may be difficult to define among species that have close relations. But, being sure that there are limits, that science, in looking for law and order written out in legible characters, is not in fruitless search, we need not despair of discovering them. The zoölogist, gathering shells or mollusks from the coast of eastern America and that of Japan, after careful study, makes out his lists of identical species, with the full assurance that species are definite and stable existences; and he is even surprised with the identity of characters between the individuals of a species obtained from so remote localities. And as he sees zoölogical geography rising into one of the grandest of the sciences, his faith in species becomes identified with his faith in nature and all physical truth.

If, then, we may trust this argument from general truths to special, — general *truths* I say, for general principles as far as established are truths, — we should conceive of a species from the potential point of view, and regard it as —

a. A concentred unit of force, an ineffaceable component of the system of nature; but

b. Subject to greater or less librations, according to the universal law of mutual reaction or sympathy among forces.

And, in addition, in the *organic* kingdom,

c. Exhibiting its potentiality not simply or wholly in any existing condition or action, but through a cycle of growth from the primal germ to maturity, when the new germ comes forth as a repetition of the first to go another round in the cycle and perpetuate the original unit; and, therefore, as follows from a necessary perpetuity of the cycle —

d. Exhibiting identity of species among individuals, by perpetuated fertile intermixture in all normal conditions, and non-identity by the impossibility of such intermixture, the rare cases of continuation for one or two generations, attesting to the stability of the law, by proving the effort of nature to rid herself of the abnormity, and her success in the effort.

e. The many like individuals that are conspecific do not properly constitute the species, but each is an expression of the species in its potentiality under some one phase of its variables; and to understand a species, we must know its law through all its cycle of growth, and its complete series of librations.

We should therefore conceive of the system of nature as involving, in its idea, a system of units, finite constituents at the basis of all things, each fixed in law; these units in inorganic nature as adding to their kinds by combinations in definite proportions; and those in organic nature adding to their numbers of representative individuals, but *not* kinds, by self-reproduction; and all adding to their varieties by mutual reaction or sympathy. Thus, from the law within and the law without, under the Being above as the Author and sustainer of all law, the world has its diversity, the cosmos its fulness of beauty.

I would remark again, that we must consider this mode of reaching truth, by reasoning from the general to the special, as requiring also its complement, direct observation, to give unwavering confidence to the mind; and we should therefore encourage research with a willingness to receive whatever results come from nature. We should give a high place in our estimate to all investigation tending to elucidate the variation or permanence of species, their mutability or immutability; and at the same time, in order that appearances may not deceive us, we should glance towards other departments of nature, remembering that all truth is harmonious, and comprehensive law the end of science.

A word further upon our conceptions of species as realities. In acquiring the first idea of species, we pass, by induction, as in other cases of generalization, from the special details displayed among individuals to a general notion of a unity of type; and this general notion, when written out in words, we may take as an approximate formula of the species. One system of philosophy thence argues that this result of induction is nothing but a notion of the mind, and that species are but an imaginary product of logic; or at least, that since, as they say (we do not now discuss this point), genera are groupings without definite limits which may be laid off variously by different minds, so species are undefined, and individuals are the only realities — the supposed limits to species being regarded as proof of partial study, or a consequence of a partial development of the kingdoms of nature. Another system infers, on the contrary, that species are realities, and the general or type-idea has, in some sense, a *real* existence. A third admits that species are essentially realities in nature, but claims that the general idea exists only as a result of logical induction.

The discussion in the preceding pages sustains most nearly the last view, that species are realities in the system of nature while manifest to us only in individuals; that is, they are so far real, that the idea for each is definite, even of mathematical strictness (although not thus precise in our limited view), it proceeding from the mathematical and finite basis of nature. They are the units fixed in the plan of creation, and individuals are the material expressions of those ideal units.

At the same time, we learn, that while species are realities in a most important and fundamental sense, no comprehensive type-idea of a species can be represented in any material or immaterial existence. For while a species has its constants, it has also its variables, each variable becoming a constant so far only as its law and limits of variation are fixed; and in the organic kingdoms, moreover, each individual has its historic phases, from the germ through the cycle of growth. The general idea sought out by induction, therefore, is not made up of *in*variables. Limited to these, it represents no object, class of objects, or law, in nature. The variables are a necessary complement to the invariables; and the complete species-idea is present to the mind, only when the image in view is seen to be ever changing along the lines of variables and development. Whatever individualized conception is entertained, it is evidently a conception of the species in one of its phases, — that is, under some one specific condition as to size, form, color, constitution, etc., as regards each part in the structure, from among the many variations in all these respects that are possible: mind can picture to itself individuals only, and not species, and one phase at a time in the life of an organic individual, not the whole cycle.

We may attempt to reach what is called the typical form of a species, in order to make this the subject of a conception. But even within the closest range of what may be taken as typical characters, there are still variables; and moreover, we repeat it, no one form, typical though we consider it, can be a full expression of the species, as long as variables are as much an essential part of its idea as constants. The advantage of fixing upon some one variety as the typical form of a species is this, — that the mind may have an initial term for the laws embraced under the idea of the species, or an assumed centre of radiation for its variant series, so as more easily to comprehend those laws.

Again, abrupt transitions, and not indefinite shadings, have been shown to be the law of nature. In proceeding from special characters to a general species-idea, nature gives us help through her stepping-stones and barriers. In former times, man looked at iron and other metals from the outside only, and searching out their differences of sensible characters, gradually eliminated the general notion of each, by the ordinary logical method of generalization. But science now brings the elements to the line and plummet, and reaches a fixed *number* for iron and other elements as to chemical combination, etc. By this means, the studying out of the idea of a species seems almost to have escaped from the domain of logic into that of direct trial by weights and measures. It is no longer the undefined progress of simple reason with a mere notion at the end, but an appeal to definite measurable values, with stable numbers at bottom, fixed in the very foundations of the universe. So, in the organic kingdoms, where there is, to our limited minds, still greater indefiniteness in most characters, the barrier against hybridity appears to stand as a physical test of species. We are thus enabled, in searching into the nature of a species, to strike from the outside detail to the foundation law.

The type-idea, as it presents itself to the mind, is no more a subject of defined conception than any mathematical expression. Could we put in mathematical terms the precise law, in all its comprehensiveness, which is at the basis of the species iron, as we can for one of its qualities, that of chemical attraction, this mathematical expression would stand as a representative of the species; and we might use it in calculations, precisely as we can use any mathematical term. So, also, if we could write out in numbers the potential nature of an organic species, or of its germ, including the laws of its variables, this expression would be like any other term in the hands of a mathematician; the mind would receive the formula as an expression for the species, and might compare it with the formulas of other species. But, after all, we have here a mere mathematical abstraction, a symbol for an amount or law of force, which can be turned into conceptions, only by imagining (supposing this possible) the force in the course of its evolution of concrete realities, according to the law of development and laws of variations embraced within it.

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2. On the supposed Uniformity of Cranial Type, throughout all Varieties of the American Race. By Daniel Wilson, University College, Toronto, Canada West.

The universal prevalence of certain characteristics common to all the nations of the New World, and especially of a uniformity of cranial conformation throughout the entire native population of North and South America, has so often been asserted, that it is unnecessary to quote authorities in proof of this. Were such requisite, a considerable list of distinguished writers might be named who have maintained this opinion; though more careful inquiry tends to prove that the greater number of such authorities merely reaffirm the views advanced by Dr. Samuel Morton, as conclusions derived from the valuable data accumulated in his *Crania Americana*. But there is one author, at once so distinguished among American men of science, and so peculiar in the point of view from whence he has regarded the entire question of American ethnology, as to merit special attention. Professor Agassiz, in his *Sketch of the Natural Provinces of the Animal World, and their relation to the different Types of Man*, reaffirms the homogeneous characteristics and ethnic insulation of the American Indian on entirely novel and independent grounds. After defining the evidence on which the opinion is based, that *the boundaries within which the different natural combinations of animals are circumscribed on the surface of the earth coincide with the natural range of distinct types of man*, he proceeds to show that America, including both its northern and southern continent, differs essentially from Europe with Asia, or Africa, in being characterized throughout by a much greater uniformity in all its natural productions, than any thing which comparison enables us to trace in the old world. He then adds: "With these facts before us, we may expect that there should be no great diversity among the tribes of man inhabiting this continent; and indeed the most extensive investigation of their peculiarities, has led Dr. Morton to consider them as constituting but a single race, from the confines of the Esquimaux down to the southernmost extremity of the continent. But, at the same time, it should be remembered that, in accordance with the zoological character of the whole realm, this race is divided into an infinite number of small tribes, presenting more or less difference one from another."

The latest views of Agassiz, as set forth in his contribution to the *Indigenous Races of the Earth*, present us with the same opinions, advanced with additional confirmation from other data. Passing from the general zoölogical analogies in the distribution of species to the special one of the monkey, he remarks on the diversity of opinions among men of science as to the genus *Cebus*, which some zoölogists recognize as one species, others separate into two or three, while others again subdivide it into as many as ten:—"Here we have, with reference to one genus of monkeys, the same diversity of opinion as exists among naturalists respecting the races of man. But in this case, the question assumes a peculiar interest, from the circumstance that the genus *Cebus* is exclusively American; for that discloses the same indefinite limitation between its species which we observe also among the tribes of Indians, or the same tendency to splitting into minor groups, running really one into the other, notwithstanding some few marked differences, — in the same manner as Morton has shown that all the Indians constitute but one race, from one end of the continent to the other. . . . . In the old world, notwithstanding the recurrence of similar phenomena, the range of variation of species seems less extensive, and the range of their geographical distribution more limited. In accordance with this general character of the animal kingdom, we find likewise that, among men, with the exception of the Arctic Esquimaux, there is only one single race of men extending over the whole range of North and South America, but dividing into innumerable tribes; whilst, in the old world, there are a great many well-defined and easily distinguished races, which are circumscribed within comparatively much narrower boundaries."

Since the idea of the homogeneous physical characteristics of the whole aboriginal population of America, extending from Terra del Fuego to the Arctic circle, was first propounded by Dr. Morton, it has been accepted without question, and has more recently been made the basis of many widely comprehensive deductions. Philology and archæology have also been called in to sustain this doctrine of a special unity of the American race; and to prove that, notwithstanding some partial deviations from the prevailing standard, the American Indian is essentially separate and peculiar; *a race distinct from all others.*

The strong-hold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents, is the supposed uniformity of physiological, and especially of physiognomical and cranial characteristics; an ethnical postulate which has not yet been called in question.

On first visiting the American continent, and enjoying the opportunity of judging for myself of the physical characteristics of the aboriginal race of the forests, I did so under the full conviction of meeting with such a universal approximation to the assumed Normal type, as would fully bear out the deductions of previous observers, and especially of one so persevering in the accumulation of the requisite materials on which to base a legitimate result, as the author of the *Crania Americana.* When, therefore, I proceeded to open some Indian graves in Canada, and to endeavor to procure crania from others on ascertaining of their disturbance, it was solely with a view to possess myself of one or two specimens of the peculiar American type of cranium, which possessed a special interest to me from its approximation to the ancient brachy-cephalic skull, familiar to me, as found in one important class of early British barrows. It was accordingly, simply with a sense of disappointment that I found the results of repeated efforts, in different localities, supplied me with crania, which, though undoubtedly Indian, exhibited little or no trace of the rounded form, with short longitudinal diameter, so strikingly apparent in the ancient crania of Central America and the mounds. Appreciating, as I did, the invaluable labors of Dr. Morton, — which will be more fully prized, as the important science they tend to elucidate commands a wider attention and more careful study, — it did not occur to me at first to question any of the results so frequently reiterated by him, and repeatedly confirmed by the concurrence of later writers. Slowly, however, the idea has forced itself upon me, that, to whatever extent the affirmed typical form of the American cranium may prevail in other parts of the continent, the crania most frequently met with along the north shores of the great lakes are deficient in some of its most essential elements.

In order to institute such a comparison as will satisfactorily test this question, it is necessary to define the essential requisites of the American type of cranium; for, neither Dr. Morton, nor his successors, have overlooked the fact of some deviation from the supposed Normal type, not only occurring occasionally, but existing as a permanent characteristic of certain tribes, including those to which I have more particularly to refer. Dr. Morton recognized a more elongated head as pertaining to certain tribes, of which he names the Lenapé stock, the Iroquois, and the Cherokees, to the east of the Alleghany mountains; and the Mandans, Ricaras, and Assinnaboins, to the west. But such elongation he speaks of as a mere slight variation from the more perfect form of the normal skull; and he adds: "even in these instances the characteristic truncation of the occiput is more or less obvious." *

* Crania Americana, p. 69.

So also Dr. Nott, after defining the typical characteristics of the American cranium, remarks: "Such are more universal in the Toltecan than in the barbarous tribes. Among the Iroquois, for instance, the heads were often of a somewhat elongated form, but the Cherokees and Choctaws, who, of all barbarous tribes, display greater aptitude for civilization, present the genuine type in a remarkable degree. My birth and long residence in southern States have permitted the study of many of these living tribes, and they exhibit this conformation almost without exception. I have also scrutinized many Mexicans, besides Catawbas, of South Carolina, and tribes on the Canada lakes, and can bear witness that the living tribes everywhere confirm Morton's type." *

We cannot err in taking the very interesting cranium found by Dr. Davis and Mr. Squier in a mound in the Scioto valley, Ohio, as an example of the true typical head; for it is produced as such by Dr. Nott, in the "Types of Mankind," and is described, in the words of Dr. Morton, in Dr. Meigs's *Catalogue of Human Crania, in the collection of the Academy of Natural Science of Philadelphia*, issued by order of the Academy, during the present year, as "An Aboriginal American; a very remarkable head. This is, perhaps, the most admirably formed head of the American race hitherto discovered. It possesses the national characteristics in perfection, as seen in the elevated vertex, flattened occiput, great interparietal diameter, ponderous bony structure, salient nose, large jaws, and broad face. It is the perfect type of Indian conformation, to which the skulls of all the tribes, from Cape Horn to Canada, more or less approximate." As shown by the front view of this skull, it presents no trace of pyramidial conformation.

Of this skull, the measurements which involve the most essential typical elements, and so furnish precise materials for comparison, are: —

Longitudinal diameter	6.5	inches.
Parietal "	6.	"
Vertical "	6.2	"
Intermastoid Arch	16.	"
Horizontal circumference	19.8	"

So that, in fact, the cranium very closely corresponds in its measure-

* Types of Mankind, p. 441.

ments, in length, breadth, and height. Still further it may be observed, on examining the full-sized view of the skull, as given by Messrs. Squier and Davis (Pl. XLVII.), that the singular longitudinal abbreviation of this skull is nearly all posteriorly. A line drawn through the meatus auditorius externus in profile, parallel to the elevated forehead, divides it into two unequal parts, of which the anterior and posterior parts are nearly in the ratio of two to one. To this type the ancient Peruvian and Mexican crania unquestionably approximate. Of one of the former, from the Temple of the Sun, (Pl. XI.) Dr. Morton remarks: "A strikingly characteristic Peruvian head. As is common in this series of skulls, the parietal and longitudinal diameters are nearly the same," namely, longitudinal diameter, 6-1, parietal diameter, 6. So far, therefore, as such evidence goes, it appears to justify the conclusion arrived at by Dr. Morton, that the people represented by the mound skulls in his possession, "were one and the same with the American race, and probably of the Toltecan branch." *

The conformity affirmed to exist between the ancient Mexican and Peruvian skulls, and those of the modern barbarous tribes, may also be so far asserted as a partial approximation in relation to some of them, and appears to receive a fuller confirmation when carefully selected examples are referred to; as a sufficient number occur to indicate the occasional reappearance of some of the most striking typical peculiarities.

Of a similar nature is the correspondence pointed out by Dr. Nott,† between the Scioto mound skull and that of a Cherokee chief, who died a prisoner near Mobile, in 1837. In this example, in so far as can be judged from the comparison of both by drawings in profile, without precise measurements, the points of agreement are indisputable, though even here amounting to no more than an approximation. The vertical occiput of the ancient skull, — more markedly vertical in the original drawing than in the small copy, — is only partially represented in the other; the square form of the ancient profile in the coronal region becomes conoid in the modern one; and the intersecting line drawn through the *meatus* shows a very partial reproduction in the modern example, of the remarkable preponderance of anterior cere-

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* Crania Americana, p. 229.

† Types of Mankind, p. 442.

bral development, which — if not produced by artificial means — is the most singular characteristic of the ancient head.

But while acknowledging such approximation of the selected modern Cherokee cranium to the ancient type, neither the legitimate deductions following from this, nor from the other examples referred to by Dr. Nott, appear to bear out his conclusions, that not only that type "is found among tribes the most scattered, among the semi-civilized and the barbarous, among living as well as among extinct races;" but "that *no foreign race has intruded itself in their midst, even in the smallest appreciable degree.*" The examples of Cherokee heads referred to in the *Table of Anatomical Measurements* in the Crania Americana, in so far as they fairly represent the cranial characteristics of this tribe or nation, seem to indicate that the Mobile chief is an exceptional case; and this is further borne out by the special example selected by Dr. Morton, and figured in his great work; "the head of a Cherokee warrior, who was known in the army, by the name of John Waring." The following are its most characteristic measurements, exhibiting such a wide divergence from the normal type, as illustrated in that of the Scioto mound, as to substitute contrast for comparison: —

Longitudinal diameter	7.2
Parietal "	5.3
Vertical "	5.3
Intermastoid arch	14.1
Horizontal circumference	19.1

In the typical head the longitudinal, parietal, and vertical diameters closely correspond; in this, the excess of the longitudinal over the parietal and vertical diameters is such, as is rarely exceeded in the modern Anglo-Saxon, or even the longer sub-celtic head. Yet, that such an excess in the longitudinal diameter did not present to the experienced eye of Dr. Morton any striking deviation from the form of the modern Indian head, is proved by his noting of this very example: "Nor is there any thing remarkable in the form of the skull."

Bearing in remembrance, then, the partial nature of the approximation so far apparent between the ancient and modern American cranium, personal observation leads me to believe that such is to be found, — with exceptional instances of closer affinities, and also with important divergencies from the typical Indian form and character, not exceptional, but pertaining to the whole nation, — among the still numerous examples of the Algonquin stock, as represented by the Chippeways. Of these I have examined, and compared by the eye, many at widely scattered locations; on Lake Simcoe and the Georgian Bay; at Mackinaw, in Lake Huron, and at Sault St. Marie; at Ontonagon, La Point, the Apostle islands, and the St. Louis river, on Lake Superior; as well as such chance opportunities as occur in the neighborhood of Niagara Falls, and on the streets of our Canadian towns and villages. Physiognomically they present the wide and prominent mouth, high cheek-bones, and broad face, so universally characteristic of the American Indian; but they by no means present in a remarkable degree the wide and massive lower jaw, which has been noted as of universal occurrence among the Red Indians. Still more noticeable is the absence of the aquiline nose, so characteristic generally of the true Indian in contradistinction to the Esquimaux. The eye may be fully depended on for physiognomical characteristics; it is of much less value in testing variations from any assumed cranial type, especially in reference to comparatively minute divergencies of measurement. Nevertheless, their heads appear to me to be essentially brachy-cephalic, as compared with those of other tribes in part displaced by them; but — in so far as may be judged from the observation of the living head covered with the thickly matted and long, coarse hair of the Indian — they are not remarkable for vertical elevation.

The following table presents the results of an examination of six pure-breed Chippeways, at the Indian reserve on Lake Couchiching, with the addition of two others, the only examples of the same nation, given by Morton, in the *Crania Americana.* From these it will be seen that, while in the majority of them a certain approximation of the longitudinal to the parietal diameter is discernible, it is of a very partial nature, except in one instance (No. 5), where a manifest correspondence to certain relative proportions of the mound-builder type of head is apparent.

TABLE I. — CRANIAL MEASUREMENTS. — (CHIPPEWAYS.)

	Longitudinal Diameter.	Parietal Diameter.	Frontal Diameter.	Inter-mastoid Arch.	Horizon'l Circumference.
1. Joseph Shilling	7.5	6.1	5.6	14.4	22.9
2. James Inglesol (Kobseqnan) .	7.4	6.	5.	14.8	22.3
3. Jac. Crane (Now-keise-gwab) .	7.1	6.	5.4	15.4	22.1
4. Peter Jacobs (Pah-tah-se-ga) .	7.3	5.8	5.4	15.	22.6
5. Jacob Shilling	6.9	6.	5.1	14.7	22.
6. William Snake	7.1	6.	5.5	15.1	22.
7. Crania Americana, No. 683 . .	7.3	5.8	4.8	15.1	20.9
8. Crania Americana, No. 684 . .	7.2	5.5	4.3	14.8	20.2

Some of the measurements in the living head are necessarily affected by the hair, always coarse and abundant with the Indian. Others again, such as the vertical diameter, cannot be taken; but the mastoid processes are sufficiently prominent to leave very little room for error in the measurement of the intermastoid arch; and this suffices to show the very exceptional approximation of the modern Chippeway head — in so far as it is illustrated by these examples — to the ancient type, in the proportional elevation of the vertex. In the horizontal circumference some deduction must be made for the hair, to bring it to the true cranial measurement in all the six living examples.

The Chippeways have been selected for reference here, because — taking the above measurements, along with other observations — they appear to indicate a nearer approach to some of the assumed characteristics of the American cranial type, in this widely spread branch of the Indian stock, than is observable in other northern races, and especially than is apparent on an examination of skulls belonging, as I believe, to the original Huron occupants of the country around Lakes Simcoe and Couchiching, where the Chippeways more especially referred to are now settled, and of the greater part of Upper Canada, when first explored.

But the divergent characteristics noticeable in these, and still more in the crania of older Canadian graves, are by no means confined to those named, as a few examples will suffice to show. Such a radical divergence from the assumed normal type as has been already noted in Dr. Morton's selected Cherokee cranium, is no less obvious in that of the Miami, — the head of a celebrated chief, eloquent, of great bravery, and uncompromising hostility to the Whites: (*Crania Americana*, p. 182.)

Longitudinal Diameter	7.3
Parietal Diameter	5.5
Vertical Diameter	5.5
Intermastoid Arch	14.5
Horizontal Circumference	19.8

In the example of the Potawatomies, "A skull of a genuine Potawatomie, remarkable for its capacity behind the ears." (Ibid., p. 186.)

Longitudinal Diameter	7.8
Parietal Diameter	5.7
Vertical Diameter	5.3
Intermastoid Arch	16.
Horizontal Circumference	22.1

In that of the Blackfeet, the largest of two brought to Philadelphia by Catlin, and noted by Dr. Morton for its great breadth between the parietal bones. It is also very markedly pyramidal. Nevertheless, here also the longitudinal diameter is nearly two inches in excess both of the parietal and vertical diameters. (Ibid., 202.)

Longitudinal Diameter	7.1
Parietal Diameter	5.4
Vertical Diameter	5.1
Intermastoid Arch	13.8
Horizontal Circumference	19.9

So also Dr. Morton says of the Menominees: "I have received a series of Menominee skulls, embracing eight specimens. They are something larger than the average of Indian crania; and although for the most part they present a *rather oval shape*, they are all marked by a gently flattened occiput." (Ibid., 179.) A reference to the Catalogue of the Morton Collection at Philadelphia discloses the important fact, that of those marked by the shorter longitudinal diameter, Nos. 35, 44, and 563 are females.

Again, of the Delawares he remarks: "The few Delaware skulls in my possession are more elongated than is usual in the American tribes; they are also narrower in proportion in the parietal diameter, and less flattened on the occiput."

Such are some indications of data, — derived from a source altogether unexceptionable in the present argument, — which seem to render it impossible to uphold the views so repeatedly affirmed, of the physiognomical, physiological, and, above all, the cranial unity characterizing the whole ancient and modern aborigines of the New World.

I omit, meanwhile, any reference to the characteristics ascribed by Dr. Morton to the Iroquois and Hurons or Wyandots, those tribes to whom, with the greatest probability, may be assigned the crania specially examined by me, found along the shores of Lake Ontario, the north shore of Lake Erie, and on Lake Huron.

Of Indian skulls dug up within the district once pertaining to the Huron or Wyandot branch of the Iroquois stock, I had observed and cursorily examined a considerable number before my attention was specially drawn to the peculiar characteristics now under consideration, owing to my repeated rejection of those which turned up, as failing to furnish specimens of the assigned typical American head. Since then I have carefully examined and measured twenty-nine Indian skulls, with the following results:

1. Only three exhibit such an agreement with the American type, as, judged by the eye, to justify their classification as true brachycephalic crania. One of these (No. 11), a very remarkable and massive skull, was turned up at Barrie, on Lake Simcoe, with, it is said, upwards of two hundred others. It differs from all the other Canadian crânia in exhibiting the vertical occiput so very strikingly, that, when laid resting on it, it stands more firmly than in any other position. Of the Scioto Valley cranium, Dr. Morton remarks, in reference to the occiput, "Similar forms are common in the Peruvian tombs, and have the occiput, as in this instance, so flattened and vertical as to give the idea of artificial compression; yet this is only an exaggeration of the natural form, caused by the pressure of the cradle-board in common use among the American nation." I think it extremely probable that further investigation will tend to the conclusion that the vertical or flattened occiput, instead of being a typical characteristic, pertains entirely to the class of artificial modifications of the natural cranium familiar to the American ethnologist alike in the disclosures of ancient graves, and in the customs of widely separated living tribes. In the case of the Barry skull, there can be little doubt that the flat-

tened occiput is the result of artificial compression, of a much more decided nature than that of the cradle-board of the papoose.

It is not undeserving of notice here, that the example selected by Cuvier, among his "crania pertaining to the four principal types of the human species," to illustrate the American race, exhibits a strikingly marked prolongation of the occiput. It is described as: "*Crâne trouvé dans une caverne, près du Village de Maïpuré près des bords de l'Orénoque; rapporté par M. de Humboldt;*" * and suffices to indicate in some degree how far the opinion already quoted from Humboldt's *Researches* coincides with his own independent observations.

2. In addition to what has been above remarked in reference to the probable artificial origin of the supposed typical form of the occiput, assigned by Dr. Morton to the whole American race, I am struck, in the majority of the examples examined, with the total absence of any approximation to the flattened occiput. Sixteen of the crania referred to exhibit a more or less decided posterior projection of the occiput, twelve of these being markedly so, and seven of them presenting a striking prolongation of it.

3. The tendency to the pyramidal form, occasioned by the angular junction of the parietal bones, is apparent in the majority of the skulls examined. I have noted its occurrence more or less prominently in fourteen crania, of which five exhibit a strongly marked pyramidal form, extending to the frontal bone. In some, however, it is only slightly indicated, while in several it is totally wanting.

4. I am further struck with the frequency of the very partial projection, and in some examples the total absence of the superciliary ridge, a characteristic which I am not aware has been noted before. In six of the skulls carefully noted by me, this is particularly manifest, and, along with their pyramidal vertex and predominant longitudinal diameter, suggests affinities hitherto overlooked, with the Esquimaux form of skull.

5. I would also note, that whereas Dr. Morton states, as the result of his experience, that the most distant points of the parietal bones are, for the most part, the protuberances, I have only found such to be the case in two out of twenty-nine Canadian skulls. The

* *Cuvier: Le Régne Animal. Races Humaines, planches* 1 *et* 2, *pl.* 8, *fig.* 2.

widest parietal measurement is generally a little above the squamous suture.

6. The occurrence may also be noted, in several of these crania, of wormian bones of such regularity of form and position, as to constitute indications at least seemingly confirmatory of the supposed tendency to the development of an *interparietal or superoccipital* bone, first pointed out by Dr. Bellamy. This, which is a permanent cranial characteristic in some of the mammalia, is regarded by Dr. Tschudi as an osteological feature peculiar to the Peruvians, and is, he affirms, traceable in all the skulls of that race.

TABLE II. — CRANIAL MEASUREMENTS. — WESTERN CANADA (HURONS).

	1. Long. Diam.	2. Parie. Diam.	3. Front. Diam.	4. Verti. Diam.	5 Inter-mastoid Arch.	6. Inter-Mast. Line.	7. Occip. Frontal Arch.	8. Do. from Oc. prot. to root of nose.	9. Horiz. circumference.
1. Orillia . . .	7.5	5.7	4.5	5.6	15.6	4.25	15.	13.	21.1
2. " . . .	7.4	5.5	4.4	5.4	14.7	4.5	—	12.	20.6
3. Oakridges . .	7.6	5.5	4.7	6.	15.7	4.6	15.	13.7	21.2
4. " (female)	6.8	4.8	4.2	5.	13.6	4.	13.2	11.3	18.9
5. Windsor . . .	6.6	5.3	4.2	5.5	14.5	4.2	13.5	12.2	19.
6. Peterborough .	7.7	5.5	4.9	5.3	15.4	4.6	15.	13.6	21.1
7. Windsor . . .	7.	5.7	4.7	5.7	15.2	4.3	14.5	12.9	20.1
8. " . . .	7.	5.7	4.5	5.7	16.1	4.	14.4	12.4	20.1
9. " . . .	7.4	6.1	4.9	5.7	—	4.5	15.5	13.4	21.4
10. Ponetanguishene	7.8	5.6	4.6	5.9	15.5	4.5	15.6	13.5	21.3
11. Barrie	6.6	6.4	5.2	5.3	16.	4.6	14.4	12.1	20.7
12. Burlington Bay	7.	5.25	4.4	5.3	14.	4.	13.6	11.9	19.5
13. " "	7.6	5.6	4.4	5.4	15.2	4.2	14.9	12.9	20.9
14. Burwick . . .	7.2	5.1	4.4	5.6	14.3	4.3	14.7	12.4	21.
15. Tecumseth . .	7.3	5.6	4.4	5.5	14.5	4.9	14.4	12.5	20.25
16. " (fem.)	7.2	5.2	3.9	5.	14.1	3.6	14.25	12.9	19.7
17. " . .	7.9	6.	4.6	5.7	16.	3.4	16.1	14.25	22.
18. " (fem.)	7.6	5.25	4.3	5.6	14.	4.1	14.25	12.6	20.2
19. " (fem.)	7.5	5.2	4.1	5.1	13.4	4.2	14.8	13.	20.5
20. " . . .	7.4	5.6	4.6	5.5	15.	4.4	15.	13.6	20.9
21. " . . .	7.6	5.4	4.2	5.7	15.1	4.4	15.3	14.	20.9
22. Owen Sound .	7.	5.5	4.2	5.	13.8	4.	14.	12.2	19.8
23. " " .	7.3	5.3	4.25	5.25	14.4	4.2	14.25	12.4	20.4
24. " " .	7.2	5.4	3.8	5.25	14.5	3.9	14.2	12.	19 9
25. " " .	7.7	5.4	4.7	5.6	14.6	4.2	15.	13.	21.4
26. Oro	7.4	5.4	—	4.25	15.25	4.	14.9	12.4	20.4
27. Owen Sound .	7.5	5.9	5.1	5.5	15.	4.25	15.6	13.3	21.8
28. " " .	7.6	5.5	4.5	5.4	14.6	4.5	14.9	13.1	21.3
29. Oro	7.5	5.6	4.4	5.5	15.5	4.3	15.2	13.	21.4

The table of measurements of skulls procured from Indian cemeteries to the north of Lakes Erie and Ontario (Table II.), supplies some, at least, of the elements essential to the formation of a sound judgment on the question under consideration. It embraces twenty-nine examples. To these I have added, in another table (Table III.), the corresponding measurements of the skull of the celebrated Mohawk chief, Joseph Brant, Tayendanaga, from a cast taken on the opening of his grave, at the interment of his son, John Brant, in 1852. I have also further added, from the *Crania Americana*, the Iroquois and Huron examples given there, which, it will be seen, agree in the main with the results of my own independent observations; while a comparison of the two tables will be satisfactory to those who may not unnaturally hesitate to adopt conclusions based on the amount of evidence produced, adverse to opinions re-affirmed under such various forms by so high an authority as Dr. Morton, and adopted and made the basis of such comprehensive inductions by his successors.

TABLE III. — CRANIAL MEASUREMENTS (SIX NATIONS).

	1. Long. Diam.	2. Parie. Diam.	3. Front. Diam.	4. Verti. Diam.	5. Inter-mast. Arch.	6. Inter-mast. Line.	7. Occip. Front. Arch.	8. Do. from Oc. prot. to root of nose.	9. Horiz. Circumf'rence.
Mohawk: Brant . . .	7.8	6.	5.		15.6?			13 ?	22.
Oneida, Morton, No. 33 .	7.5	5.6	4.1	5.8	14.4	4.3	14.9	——	20.8
Cayuga, " No. 417 .	7.8	5.1	4.2	5.4	14.2	4.5	15.5	——	20.8
Huron, " (fem.) No. 607	6.7	5.6	4.1	5.2	14.5	3.9	14.	——	19.2
Huron, " No. 15 .	7.2	5.3	4.3	5.5	15.	4.4	14.2	——	19.8
Iroquois, " No. 16 .	7.5	5.5	4.5	5.7	15.2	4.5	15.1	——	20.8
Iroquois, " A. N. S.	7.1	5.4	4.2	5.3	14.3	4.	14.1	——	20.

The intimate relations in language, manners, and the traditions of a common descent, between those northern and southern branches of the Iroquois stock, render these two tables, in so far as they present concurrent results, applicable as a common test of the supposed homogeneous cranial characteristics of the aboriginal American, in relation to the area of the great lakes. Twenty-nine skulls, such as the first table supplies, or thirty-six as the result of both, may, perhaps, appear to be too small a number on which to base conclusions adverse to those promulgated by an observer so distinguished and so persevering as Dr. Morton, and accepted by writers no less worthy of esteem and deference. Still more may these data seem inadequate, when it is remembered that Dr. Morton's original observations and measurements embraced upwards of three hundred American skulls. But, in addition to the fact, that the measurements now supplied are only the more carefully noted data which have tended to confirm conclusions suggested by previous examinations, in a less detailed manner, of a larger number of examples, an investigation of the materials which supplied the elements of earlier inductions will show, that only in the case of the ancient "Toltecan" tribes did Dr. Morton examine nearly so many examples; while, in relation to what he designated the "Barbarous Race," to which the northern tribes belong, even in Dr. Meigs' greatly enlarged catalogue of the Morton Collection, as augmented since his death, the Seminole crania present the greatest number belonging to one tribe, and these only amount to sixteen.

In contrast to the form of head of the true American race, Dr. Morton appends to his *Crania Americana* drawings and measurements of four Esquimaux skulls, familiar to me, if I mistake not, in the collection of the Edinburgh Phrenological Society. In commenting on the views and measurements of these, he remarks: "The great and uniform differences between these heads and those of the American Indians will be obvious to every one accustomed to make comparisons of this kind, and serve as corroborative evidence of the opinion that the Esquimaux are the only people possessing Asiatic characteristics on the American continent." In some respects this is undoubtedly true; the prognathous form of the superior maxilla, and the very small development of the nasal bones, especially contrast with well-known characteristics of the American aborigines. But having had some little familiarity in making comparisons of this kind, it appears to me, notwithstanding these distinctive points, that an impartial observer might be quite as likely to assign even some of the examples of Iroquois and other northern tribes figured in the *Crania Americana*, to an Esquimaux, as to a Peruvian, Mexican, or Mound-Builder type. Compare, for example, the vertical and occipital diagrams, furnished by Dr. Morton, of the Esquimaux crania (p. 248) with those of the Iroquois and Hurons (pp. 192–194). Both are elongated, pyramidal, and with a tendency towards a conoid, rather than a flattened or vertical occipital form; and when placed along-side of the most markedly typical Mexican or Peruvian heads, the one differs little less widely from these than the other. The elements of contrast between the Hurons and Esquimaux are mainly traceable in the bones of the face — physiognomical, but not cerebral.

Taking once more their cranial measurements as a means of comparison, these, when placed along-side each other, equally bear out the conclusions already affirmed. For comparison, I select, in addition to the Scioto Valley Mound-Builder, the following, as those pointed out by Dr. Morton's own descriptions as among the most characteristic he has figured: Plate XI., Peruvian from the Temple of the Sun.— "A strikingly characteristic Peruvian head." Plate XI., C. — "Here again the parietal and longitudinal diameters are nearly equal. The posterior and lateral swell of this cranium is very remarkable, and the vertex has the characteristic prominence." Of the Mexican skulls, Dr. Morton remarks of Plate XVII.: "with a better forehead than is usual, this skull presents all the prominent characteristics of the American race,— the prominent face, elevated vertex, vertical occiput, and the great swell from the temporal bones upward;" and of Plate XVIII.: "a remarkably well characterized Toltecan head, from an ancient tomb near the city of Mexico."

TABLE IV. — COMPARATIVE CRANIAL MEASUREMENTS.

	Long. Diam.	Parie. Diam.	Front Diam.	Verti- Diam.	Inter. Mast. Arch.	Inter- Mast. Line.	Occip. front Arch.	Horiz. Cir- cumf.
Scioto Mound	6.5	6.	4.5	6.2	16.	4.5	13.8	19.8
Peruvian.	6.1	6.	4.7	5.5	16.	4.5	14.1	19.5
"	6.	5.9	4.4	5.	15.5	4.	13.2	19.
Mexican	6.8	5.5	4.6	6.	15.6	4.4	14.6	19.9
Toltecan.	6.4	5.7	4.5	5.4	14.6	4.5	13.5	20.2
Iroquois	7.5	5.5	4.5	5.7	15.2	4.5	15.1	20.8
Cayuga	7.8	5.1	4.2	5.4	14.2	4.5	15.5	20.8
Oneida	7.5	5.6	4.1	5.8	14.4	4.3	14.9	20.8
Huron	7.2	5.3	4.3	5,5	15.	4.4	14.2	19.8
Esquimaux	7.5	5.4	4.6	5.4	14.3	4.1	15.2	20.4
"	7.3	5.5	4.4	5.3	14.1	4.3	14.4	20.3
"	7.5	5.1	4.3	5.5	14.8	3.9	15.5	20.3
"	6.7	5.	4.4	5.	13.6	4.	13.9	18.9

These examples I refer to in preference to those presented in the previous table as the result of my own observations, as they are necessarily unbiased. They are the specimens of the very stock I refer to, selected or brought by chance under the observation of Dr. Morton, and included as the characteristic or sole examples of its tribes or nations, in his great work. But the same conclusions are borne out by the examples obtained within the Canadian frontiers; and they seem to me to lead inevitably to this conclusion, that if crania measuring, in some cases, two inches in excess in the longitudinal over the parietal and vertical diameters, and in others nearly approximating to such relative measurements, — without further reference here to variations in occipital conformation, — if such crania may be affirmed, without challenge, to be of the same type as others where the longitudinal, parietal, and vertical diameters vary only by small fractional differences, then the distinction between the *brachycephalic* and the *dolichocephalic* type of head is, for all purposes of science, at an end, and the labors of Blumenbach, Retzius, Nilsson, and all who have trod in their footsteps, have been wasted in pursuit of an idle fancy. If differences of cranial conformation of so strongly defined a character, as are thus shown to exist between the various ancient and modern people of America, amount to no more than variations within the normal range of a common type, then all the important distinctions between the crania of ancient European barrows and those of living races, amount to little; and the more delicate details, such as those, for example, which have been supposed to distinguish the Celtic from the Germanic cranium, the ancient Roman from the Etruscan or Greek, the Sclave from the Magyar or Turk, or the Gothic Spaniard from the Basque or Morisco, must be utterly valueless. If external circumstances or the progress of civilization exercise any influence on physical form, a greater diversity of conformation is to be looked for in Europe than among the Indians of America, where, as in Africa, nearly the same habits and modes of life have characterized the whole "Barbarous Race," throughout the centuries during which Europe has had any knowledge of them. But, making full allowance for such external influences, it seems to me, after thus reviewing the evidence on which the assumed unity of the American race is founded, little less extravagant to affirm of Europe than of America, that the crania everywhere and at all periods, have conformed, or even approximated to, one type.

As an hypothesis, based on evidence accumulated in the *Crania Americana*, the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalizations relative to the Southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the Northern continent, the conclusions still go far beyond any thing established by absolute proof; and the subsequent labors of Morton himself, and still more of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing by further evidence a novel and ingenious hypothesis.

Dr. Latham, after commenting on the manifest distinctions which separate the Esquimaux of the Atlantic from the tribes of the American aborigines lying to the south and west of them, as elements of contrast which have not failed to receive full justice, adds: "It is not so with the Eskimos of Russian America, and the parts that look upon the Pacific. These are so far from being separated by any broad and trenchant line of demarcation from the proper Indian, or the so-called Red Race, that they pass gradually into it; and that in respect to their habits, manner, and appearance, equally. So far is this the case, that he would be a bold man who should venture, in speaking of the southern tribes of Russian America, to say, *here the Eskimo area ends, and here a different area begins*." * The difference thus pointed out may be accounted for, to a considerable extent, by the diverse geographical conformation of the continent, on its eastern and western sides, which admit in the latter of such frequent and intimate intercourse as is not unlikely to lead to an intermixture of blood, and a blending of the races, however primarily distinct and diverse. The evidence presented here, however, refers to tribes having no such

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* Varieties of Man, p. 291.

intercourse with the Esquimaux, and distinguished from them by many important characteristics, in manners, social habits, and external physiognomy. Nevertheless, if these conclusions, deduced from an examination of Canadian crania, are borne out by the premises, and confirmed by further investigation, this much at least may be affirmed: that a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi; and in so far as the northern differ from the southern tribes, they approximate more or less, in the points of divergence, to the characteristics of the Esquimaux: — that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent ethnologists to be physically a Mongol and Asiatic, if philologically an American.

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3. On the word Celt. By J. P. Lesley, of Philadelphia.

If the new meaning which I propose for the word Celt be accepted, it will place it on a different ethnological footing. The Celtic family has been divided into two groups, the Gallic, and the Cimbric; and Gauls, Gael, Galli, κελται, γαλαται, cymri, κιμμεριοι, have been pronounced, more or less distinctly, to be Celtic cognomens. Historical and hypothetical migrations have been called in to account for the extensive range of these and similar names. Yet no one seems to feel the identity with them of such extralimital names as Koord and Chaldee, nor to recognize the evidently identified *Culdee* as any thing more than a religious title.

An analysis of the word, by the following formula, will put us on the track of its aboriginal use.

$$K.B.L = K'L \text{ and } K.B.L.t = K'Lt.$$

the final *t* being an attributive or possessive form. It is a law of pronunciation that dissyllables become, in process of time, monosyllables by losing either the initial, medial, or final, whichever be the weakest, or, according to another law, whichever be most difficult for the organs of the particular people in question to pronounce. The final disappears in rapid speech, and when it is a dental, and especially a liquid, as in Hebrew, bar, English, boy, etc. The medial is dropped sometimes when a guttural, but most commonly when a labial. The result is a diphthong; and the artistic construction of the Sanscrit diphthongs is merely an exception to this more general, and, in fact, universal rule. The difficulty of telling in all cases precisely what radical has dropped out from the middle of the diphthong is great, as for example in the English, Flail, which may come from Flabellum, Flap, or from Flagellum, Flügel. But it remains true that whenever, in modern words, we see a diphthong written, or a broad u or o or double oo representing a diphthong, we must begin by supposing the loss of a middle radical, and commonly a labial. The initial is dropped by some recondite organic influence, hard to make out, and also by virtue of a peculiar aboriginal mythologic influence, which it is the object of this paper to make clear. Often, we have all these abbreviations in one set of words, as cap, pate, and hat, are seen to be all contractions of caput, when we rightfully view caput as = cover. It is a curious characteristic of the Polynesian and Australian dialects, that they drop the initial whether it be a guttural or dental. Thus the Sydney *kabara* and Muruya *Kapan* become the Peel river *Bura*, the lake Maquarie *Walong*, the Bathurst *Balang*, the Wellington *Budian*, all meaning *head*, caput, κεφαλη, in Basque *Burua*. I have selected this word, however, not from any peculiarity in exhibiting this law, but from its very extensive range, and because it lies so near the analysis of the word Celt. To show this, I will throw some of its forms into the following table, by which the changes will be patent to the eye.

THE FORMS OF THE WORD HEAD.

Australian,	KA — —			
Mexican,	OK — —			
Chinese,	HEE — —			
Sanscrit,		kaKU-Bh —	KA-PA-LA	
German,		KO-PF —	HA-UP-T	
Latin,			CA-PU-T	
Greek,			KE-ΦA-ΛE	— BU-Dian
Australian,			KA-PA-N	— BA-Long
			KA-BA-RA	— WA-Long
				— BU-RA

Basque,	— BU-RUa	
Robber dialect of Italy,	— BO-RE-La	
Delaware and other neighboring North American Indians,	— WI-hL — WI-LA — WY-ER	
English,	— PA-Te	
Welsh,	— PE-N	— — UTa.
Polynesian,	— PE-Nu	— — ULa.

Here we have the central, typical, and full form, face to face with the lateral, partial, or suppressed forms, and it is to be remarked that this central form occupies, geographically and historically, the central position. It is the Sanscrit and Greek which gives us the pure KA-BA-R form; and yet its original life was vigorous enough to make it reappear, even in Australia. Caput and kapan, seem at first mere aberrants ($L = R \times D = T \times N$), until we introduce the Caucasian dialects, which are, perhaps, the best analytical reagents we possess, when the final in caput is seen to be an affix, as we have in Ingoushi K-WA-R-té, contracted into K'OR-té, and in Tchetchen K'AR-té and K'OR-te. We may, therefore, rewrite our series thus:—

KA"	kaKUBh'	KAPALa	'BURa	
OC"	KOPF'	KEFALe	'BURua	''ULa.
HEE"		KABARa	'WILa.	''U'tu.
		HAUP' t	'WyeR	
		CAPU' t	'BORela	
		KAPA' n	'BaLong	
			'WALong	
			'BU'dian	
			'PA'te	
			'PE'n	
			'PE'nu	

Not to ignore, however, the transmutation L,R $\times$ D,T,N, we may consider 'ula, 'utu, bura, pate, pen, to be equivalents, and even caput and κεφαλη; but there is no alternative with the Caucasian kawarté, and the Australian terminal nasals. They prove the full form to be cabart, or cabalt, and the aboriginal meaning follows,—Belonging to or representing the cabar, i. e., cabalistic, — which the human head in all ages has been, and hence the tonsure which represents the emerged summit of the mountain surrounded by the waves of hair.

It will be seen that the point of this analysis lies in the contraction *kawarié*, *karté*. The medial labial is gone. It is also gone in Coptic, KAEa, Sanscrit, S-CHIRa, English, S-CULL. It falls away, in fact, in all uses and applications of this curious root-word *kabulo*. Arabic *Gibel* becomes French *Col.* Wellington *Giber* (man) becomes Sydney *kore*. Lesgian *chabbar* becomes Kisti *ker* and English *clay*. Lesgian *khimmir* becomes English *child*. Lesgian *gubur* (neck) becomes Georgian *kaili*, French *Col.* Cabriolet becomes curricle. Cowries, the Berbers call kurdi. In all languages and places we must work with the formulæ,

$$KBR = K'R,\ KBL = K'L,\ KBN = K'N,\ KBLt = K'Lt,$$

and its affiliated formulæ which I will not discuss here,

$$TBR = T'R,\ TBL = T'L,\ TBD = T'D,\ etc.$$

Both are essentially mythological, and represent those cabalistic mysteries which have always centered round the trimurti symbol of the east, the ship-mountain-flood, Vishnu Brahma Siva, UAM, which ought to be written $\left\{\begin{matrix}U\\A\\M.\end{matrix}\right.$ The word Celt expressed this symbol, its meanings, representatives, priesthood, worshippers, etc.

This can be set in a clear light by a few illustrations. Every thing representing a ship was called Celtic; a *coble* is a boat; canoe, kahn, is the contracted form. A camel is the ship of the desert. Every thing resembling the motion of a ship received the same epithet; a horse was called caballus; a limping man was said to hobble, contracted, by the rule above, into halt. The peculiar wavy motion of the s-quir-il probably gave it its name, as much as its upright posture, with folded hands, at the mouth of its burrow.

Every thing representing the mountain was cabalistic. Its Shemitic name is gibel (every thing huge and strong and round was called geber, great, gibbous). Its miniature is called a *ca'ern;* it *covers* the caer, or shrine, the *cavern*, which, as the *chamber* of judgment and punishment, was called the *carcer*, *ja'il*, the *κοιλον*, *cœlum*, *hollow*, *hölle*, *hell*, *cell*, or *hole*. When mountain tops emerged from the sea they were particularly Celtic, and hence so many islands called Cypris, Capri, Cabra, etc. Trees, like mountains, were Celtic, as the Cypress of the grave-yard, and the Gopher of which tradition makes the ark (κιβωτος). The Fir or Pine has only lost the KA- prefix. The metal of the caern builders was *copper*, the Celtic metal par excellence. It was only to distinguish this mountain class of Celtic things from the ship class, that a later (?) use of T for K as a prefix became common, in TABOR, TOR, TOWER, ταυρος, Thurm, Tumulus, Dome, Tom, etc.

The third class of Celtic words bear reference to the third element of the Trinity symbol, the M water. Cobalt was so called from its ultra marine color. It was by the river Chebar, descending directly from Ararat, that Ezechial had all his visions. The Kelpies are water spirits as the Kobolds are mountain sprites, and we have kelp explained in the Hebrew, KLB, κυων, hound, canis (*KBN*= *K'N*), and by the dog-headed priests of Egypt. What dogs are to men, priests are to the gods, and such was Caleb to Joshua. It was now this reference to the human part of the mystery, to the spiritual beings involved in the event, to the Noachidæ as fathers and deities, and their enemies, the waves, personified into demons, that fixed the use and meaning of the epithet Cabalistic, Celtic, so deeply in Ethnology. Wherever the story of the Cabiri went, the tellers of it were guebers, capulets, ghibelines, gael. When the old pure arkism was driven into mountains and peninsulas by heresies, and received its name of bar-bar ism, its priests and people were called giaurs, infidels (i. e. to the heresies), men of Cimmerian darkness, Cimbri, Welsh, strangers, and enemies. It is significant that the aborigines of both sides of the Atlantic bear this title in almost identical and in the purest and completest forms, namely, the Basques (Escabaras as they call themselves) and the Esquimaux, while the only ethnological connection between them is a very doubtful one, through the Fins. And it is equally significant that this complete form S-CA-BA-RA, when lawfully contracted, denominates the *Scots* and *Scyths*, two equally imperfectly related races.

The fact is that when Cabar became a name for every arkite symbol discovered in nature or constructed by priestly art, and the sum total of such symbolic objects, natural and artificial, was grouped under the mystical designation of the Cabala, the lore which grew up with successive generations of bards, and moved across the face of the earth from century to century with missionary zeal from the original arkite centre wherever that may have been, into the most distant islands of the oceans, has left its traces in our day in all languages, and has fixed the epithet Celtic to innumerable mountains, buildings, parts of the human frame, instruments of labor and pleasure, and intellectual ideas. This is the only true meaning which it has. It can have no true ethnological significance, because its propagation must have ignored the limitations of race and nation in every age. No race or nation was peculiarly Celtic, except so far as they received the Cabala, and favored the Druids or Cabalists, who were distinguished from the people whom they taught what they pleased and ruled as they chose by a peculiar language, which itself must have had its own principle of growth; a different principle from that of the vernacular tongues in the midst of which it received its diverse developments. Even to this day a man who speaks unintelligibly is said to gobble, jabber, speak gibberish; birds are said to gobble; frantic persons to cut *capers*, not from imitating goats (*CaPeR* = *Go'aT*), but dervishes. It was thus that the priests and then their followers came to be called Chaldees in Assyria, and Culdees in Britain, and everywhere Celts. The Free Masons claim Solomon as the founder of their order, and strange enough, he, or the writer who assumes his place, twice calls himself a Celt. It is surprising that every meaning has been given to the word קהלת, *Qelt*, in the first and twelfth chapters of Ecclesiastes, but the simple meaning involved in spelling the word without the masoretic punctuation. As if to leave no doubt of its relationship it is spelt with Q, opening up its whole etymology; Q-ELT, CU-ELT, CaWeLt, Cabalist; Ani Coheleth as it is commonly pronounced, I the preacher, is literally and simply Ani qelt, I Celt.

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4. Laws of Descent of the Iroquois. By Lewis H. Morgan, of Rochester, N. Y.

The institutions of the Iroquois were founded upon the family relationships; in fact, their celebrated league was but an elaboration of these relationships into a complex, and even stupendous system of civil polity. At the base of this system were their laws of descent.

They were unlike both the civil and the canon law; but yet were original and well defined. The chief differences were two: first, descent among the Iroquois followed the female line, or passed through the mother, while in each of the former systems it follows the male, or passes through the father. In the second place, the collateral lines, with the Iroquois, were finally brought into or merged in the lineal; while, in the other cases, every remove from the common ancestor separated the collateral lines from the lineal, until after a few generations actual relationship ceased among collaterals.

To bring out distinctly this code of descent, it will be necessary to give a brief explanation of the division of the Iroquois into tribes, the union of the several tribes into one nation, and of the several nations into one league. Without a reference to their civil organization, it would be impossible to present it in an understandable form.

In each of the five nations who composed the original league, there were eight tribes, named as follows: Wolf, Bear, Beaver, and Turtle; Deer, Snipe, Heron, and Hawk. The Onondaga nation, therefore, was a counterpart of the Cayuga, each having the same number of tribes, and of the same name; so also, interchangeably, of the Oneida, the Mohawk, and the Seneca nations. In effect, the Wolf tribe was divided into five parts, and one fifth part of it placed in each of the five nations. The remaining tribes were subjected to the same division and distribution. Between the individual members of the Wolf or other tribe thus divided, or, in other words, between the separated parts of each tribe, there existed the tie of consanguinity. The Mohawk of the Turtle tribe recognized the Seneca of the Turtle tribe as a relative, and between them existed the bond of kindred blood. In like manner the Oneida of the Hawk tribe received the Onondaga or the Cayuga of the same tribe as a relative, not in an ideal or conventional sense, but as actually connected with him by the ties of consanguinity. Herein we discover an element of union between the five nations, of remarkable vitality and power. A cross-relationship existed between the several tribes of each nation and the tribes of corresponding name in each of the other nations, which bound them together in the league with indissoluble bonds. If either of the nations had wished to cast off the alliance, it would have broken this eightfold bond of consanguinity. Had the nations fallen into collision with each other, it would have brought Hawk tribe against Hawk tribe —

in a word, brother against brother. The history of the Iroquois exhibits the wisdom of these organic provisions; for, during the long period through which the league subsisted, they never fell into anarchy, nor even approximated to a dissolution from internal disorders.

Originally, with reference to marriage, the four tribes first named were not allowed to intermarry; neither were the last four. In their own mode of expressing the idea, each four were brother tribes to each other, and cousins to the other four; but either of the first four could intermarry with either of the last four. Thus Hawk could intermarry with Beaver, Heron with Turtle, Deer with Wolf; but not Beaver with Turtle, nor Hawk with Heron. This was the ancient law; but in process of time its rigor was relaxed, until finally the prohibition was confined to the tribe of the individual. At no time in the history of the Iroquois could a man marry a woman of his own tribe, even in another nation. All of the members of a tribe were within the prohibited degrees of consanguinity; and to this day, among the descendants of the Iroquois, this law is religiously observed. Husband and wife, therefore, were in every case of different tribes. The children were of the tribe of the mother. Here, then, we discover the central idea of their laws of descent: to place the father and mother in different tribes, and to assign the children to the tribe of the mother. Several important results followed, of which the most remarkable was, the perpetual disinheritance of the male line. As all titles, as well as property, descended in the female line, and were hereditary in the tribe, the son could never succeed to his father's title of sachem, nor inherit even his tomahawk.

A tribe of the Iroquois, it thus appears, was not, like the Grecian and Roman tribes, a circle or group of families, for two tribes were necessarily represented in every family; neither, like the Jewish, was constituted of the lineal descendants of a common father; on the contrary, it involved the idea of descent from a common mother; nor has it any resemblance to the Scottish clan, or to the canton of the Switzer. It approaches, however, nearest to the Jewish. Denying geographical boundaries, a tribe of the Iroquois was composed of a part of a multitude of families, as wide spread as the territories of the race, but yet united together by a common tribal bond. The mother, her children, and the descendants of her daughters in the female line, would, in perpetuity, be linked with the fortunes of her own tribe;

while the father, his brothers and sisters, and the descendants in the female line of his sisters, would be united to another tribe, and held by its affinities. No circumstances could work a translation from one tribe to another, or even suspend the nationality of the individual. If a Cayuga woman of the Hawk tribe married a Seneca, her children were of the Hawk tribe and Cayugas, and her descendants in the female line, to the latest posterity, continued to be Cayugas and of the Hawk tribe, although they resided with the Senecas, and by successive intermarriage with them had lost nearly every particle of Cayuga blood. Neither could intermarriage with one of a foreign nation confer the Iroquois nationality upon the wife or children of the marriage, and the same *vice versa*. If a Mohawk married a Delaware woman, she and her children were not only Delawares still, but ever continued aliens, unless naturalized as Mohawks, with the forms and ceremonies prescribed in cases of adoption.

Such property as they possessed, as planting lots, orchards, articles of apparel, etc., descended in the female line; that is to say, the wife and children took nothing from the father and husband, as they were of another tribe, except it was given to them by the deceased before his death, in the presence of witnesses. The property went to the brothers and sisters of the deceased, or to the children of the sisters. The property of husband and wife was kept distinct during the marriage, and held by separate ownership; and upon the death of the mother, her property was inherited by her children. Usually, planting lots, orchards, etc., belonged to the female. In case of divorce, each took their separate effects. The children belonged to the mother, and the authority and control of the father over them ceased from the moment of separation.

Here, then, we find, as the principal feature of this code of descent, the deflection of the inheritance from the son, and its bestowal upon the brother or nephew of the deceased. The great object of this singular provision had relation to the descent of the title of chief, or sachem, which will be elsewhere considered.

The next feature of importance in their system of descent was the breaking up of the collateral line, by merging it in the lineal, whereby the number of those who were bound together by the nearer family ties was largely multiplied. In three removes from the common ancestor, in most cases, and in four, absolutely, this result was effected:

It was accomplished by bringing the degrees of relationship nearer to each other than they are in the civil or the canon law. Thus a mother and her sisters stood equally in the relation of mothers to the children of each other; the grandmother and her sisters were equally grandmothers, and so up in the ascending series. The children of two sisters were the children equally of each other, and the grandchildren of the one were the grandchildren of the other, and so down in the descending series. On the side of two brothers the degrees were reckoned in the same manner. A difference, however, was made between the children of a brother and the children of a sister, in their relationship to each other. Thus the children of two sisters were brothers and sisters to each other; they were all of the same tribe. So also were the children of two brothers, although they might be of different tribes. But the children of a brother and the children of a sister were cousins, as in the civil law; they were necessarily of different tribes. The sister was aunt to the brother's children, and the brother was uncle to the sister's, and the children of these nephews and nieces were the grandchildren equally of each. Again, the cousins themselves were interchangeably uncles and aunts to the children of each other, and grandfathers and grandmothers to their children. By this simple process of reckoning degrees, the subdivision of a family into collateral branches was rendered impossible. A cousin who stands in the fourth degree of the civil law was the most remote collateral recognized in their code of descent, or, rather, allowed from the *lineal* line.

To render these degrees of relationship intelligible, it must be remembered, that a part only of the kindred of an individual were of the same tribe with himself. Thus, Sa-go-ye-wat-hä, or Red Jacket, was of the Turtle tribe of the Seneca nation. His brothers and sisters, his mother and her brothers and sisters, and his maternal grandmother and her brothers and sisters, were necessarily of the Turtle tribe; so also were the children of his sisters, and thus down through the female line. But his father, and his brothers and sisters, and his paternal grandfather, and his brothers and sisters, would be of a different, and might be of several different, tribes; so, also, his sons, and the children of his sons, would be of a different tribe, unless these sons should marry back into the Turtle tribe, against which there was no prohibition.

These laws of descent were not confined to a special class, but were of universal application; and to this day, among the descendants of the ancient Iroquois, they are preserved and recognized unchanged, and are as familiar to the rudest Indian as the alphabet is to us.*

To understand the practical use of this code of descent in its most important relation, namely, the descent of the title of sachem, it will be necessary to examine briefly the structure of the League of the Iroquois. At the institution of the league, fifty permanent sachemships or hereditary titles were created, and named. They were then distributed among the nations as follows: nine of them were assigned to the Mohawk, nine to the Oneida, fourteen to the Onondaga, ten to the Cayuga, and eight to the Seneca nation. These titles were made hereditary in certain tribes, some of which received two or more, and others none. These sachemships could never pass out of the tribe to which they belonged, except with its extinction. While the office of sachem was absolutely hereditary in the tribe, it was, at the same time, elective as between certain of the male relatives of the deceased sachem of the same tribe with himself.

The title of sachem was surrounded by insuperable barriers against the designs of talented and ambitious men, for reasons of policy; and the safeguards against usurpation were too deeply integrated in their institutions to be overcome or superseded. How this was accomplished was, for a long period, difficult to be understood; but the intricacy is removed by the single fact, before stated, that the title was hereditary in the tribe, but elective as between certain of the male relatives of the deceased sachem. It will not be necessary to explain minutely how the choice was made, further than to say, that, if the title belonged to the Wolf tribe, the new sachem must be "raised up," to use their own expression, from the same tribe. As the son of the sachem was of

* The following are the names of the several degrees of relationship recognized among the Iroquois, in the dialect of the Senecas. ä as in art. ă as in at.

Hoc-sote', Grandfather.	Hoc-no'-seh, Uncle.
Uc-sote', Grandmother.	Ah-geh'-huc, Aunt.
Hä'-nih, Father.	Hä-yan-wan-deh', Nephew.
No-yeh', Mother.	Kä-yan-wan-deh, Niece.
Ho-ah'-wuk, Son.	Dä-ya-gwä-dan-no-dä, Brothers and Sisters.
Go-ah'-wuk, Daughter.	
Kä-yä'-dä, Grandchildren.	Ah-gare'-seh, Cousin.

another tribe, he was out of the line of succession; but his brothers were of the Wolf tribe, and so were his sister's sons; hence we find that the succession fell upon a brother of the deceased ruler, or upon a nephew. Between a brother of the deceased and the son of a sister there was no law establishing a preference; neither as between several brothers on one side, or several sons of a sister on the other, was there any law of primogeniture. They were all equally eligible, and the law of election came in to decide between them. The choice was made by the wise men and matrons of the tribe; and among the latter the mother of the deceased ruler exercised a decisive influence.

Upon the decease of a sachem, and the choice of a successor, a council of all the Sachems of the League was convened to "raise up" the new ruler, and invest him with his title. To this council belonged the exclusive power of investing with the office; and no one could become a sachem in fact, until this ceremony of investiture was performed. These councils lasted several days, and were attended with many forms and ceremonies. They are still held in western New York as often as each alternate year.

These sachems were the rulers of the people, partly by elective, and partly by hereditary, right; but their duties and authority were confined exclusively to the affairs of peace. When assembled together, they formed the general council of the league, and possessed, in themselves, the executive, legislative, and judicial powers of the Commonwealth. In the same manner the several sachems of each nation composed the national council, which exercised a separate government over all the affairs of their respective nations, such as did not relate to the general welfare.

Many years after the formation of the league a new office was created, the office or title of *chief*. It was of lower rank than that of sachem, and was not hereditary. It was in the strict sense elective, and the reward of merit, and ceased with the life of the individual. To this class the most distinguished of the war captains and orators of the Iroquois belonged; among them, Tä-yen-dä-nae-ga, or Joseph Brant, and Sa-go-ye-wat-hä, or Red Jacket. At the present time the Seneca nation, in western New York, have eight sachems, as of old, who hold their titles by the original tenure, and about seventy chiefs, who hold by election.

It thus appears that the government of the Iroquois was an hereditary oligarchy, or the rule of a few select men, under limited and peculiar laws of descent. Unlike the oligarchies of civilized states, which have proved the most unstable of all forms of government, that of the Iroquois had a systematic, even constitutional, structure; and endured from age to age unbroken and unchanged, growing with increasing strength until the advent of the Saxon race.

It is an interesting fact, that the sachems of the Iroquois at the present day, although the league is dismembered, and the nations are scattered, still bear the same individual names which were borne by their predecessors at the establishment of the league. Thus Ho-no-we-nă-to, which means "Keeper of the Wampum," was the name given to one of the fourteen original Onondaga sachems. All of his successors, through many generations, down to the present Ho-no-we-nă-to, now at Onondaga, have held the same title, and borne the same name. Do-ne-ho-gä-weh, the "Keeper of the Door," was the name of one of the eight original Seneca sachems. This title, in like manner, has been held by all of his successors, down to the present day. Ely S. Parker, an educated Seneca, at the present time in the civil service of the United States, now holds this sachemship. When he was raised up, a few years since, his former name, Hä-seh-no-an-da, was "taken away," to use again their mode of expression, and the name Do-ne-ho-gä-weh bestowed in its place, by which alone he is now known. The office of sachem, therefore, is a title of nobility, but descending in the female line, instead of the male, and having attached to it the authority and powers of an hereditary ruler of the Iroquois.

Such is a brief outline of their laws of descent. The closer they are inspected, the more the unity and clearness of the system appears; but to enter into minute details would not be appropriate to the present occasion. In itself considered, this code of descent is of very little importance; and although it furnishes a singular illustration of the cast of the Indian intellect, its presentation here could not be justified on the grounds of mere curiosity.

This brings me to the precise object which led to the presentation of this subject; and it is this: — Can this code of descent, or any other original, well-defined, Indian institution, be used as a test of the truthfulness of history? In the second place, can it be employed as an instrument in the attempt to solve the great problem of the origin of our Indian races? If it can be used for either purpose, that fact invests it with a high degree of importance.

Let us observe that the primary institutions of a people are necessarily permanent from age to age, and only change when the whole constitution of society is changed. While in the nomadic or hunter state, institutions of this character are as permanent as the state in which they are developed. It is only by the entire and absolute transmutation of a race from the hunter to the civilized condition, that such institutions can be eradicated; and even then they rarely disappear entirely, as witness the tribes of the Athenians. Not even language itself will be found to be more stable than the domestic institutions within certain limits. Now it is very possible that a primary institution of an aboriginal people, may, if diligently explored among the races of the earth now living in the nomadic or hunter state, lead to important deductions in relation to the ultimate affinity, as well as origin, of generic races. And these results would repose with solidity upon the necessary permanence of such institutions, as this fact of permanence excludes the idea of accidental coincidence.

The uses of this code of descent, as a test of history, may be illustrated by the following example, but in a case in which the truth is of but little importance. Mr. Stone, in his Life of Brant (I. 8–14), thought it desirable to prove that Brant was a sachem; and therefore a man of position and influence among the Iroquois. To do this he attempts to show that his father was a Mohawk sachem of the Wolf tribe, and thus devolves the title upon him by descent. The names of the two persons, each of whom was reputed to be the father of Brant, are given, and they are both claimed, by the author, to be sachems; but it so happens that neither of their names, nor that of Brant, is found in the authentic list of the nine Mohawk sachems. And, further, had he succeeded in showing that his father was a sachem, that fact would have insured Brant's disinheritance.

A more interesting example is found in the descent of the pretended crown of Mexico, or of the Aztec monarchy. According to current histories, the crown descended either to a *brother* or to a *nephew* of the deceased emperor, and not to his *son*. As between his brother and his nephew it was elective; but under what laws or regulations the election was made, no very satisfactory account is given. Mr.

Prescott, in his work upon the Conquest of Mexico (I. 23), says, "The government was an elective monarchy. Four of the principal nobles, who had been chosen by their own body in the preceding reign, filled the office of electors, to whom were added, with merely an honorary rank, the two royal allies of Tezcuco, and Tlacopan. The sovereign was selected from the *brothers* of the deceased prince, or in default of them, from his *nephews*. Thus the election was always confined to the same family. The candidate preferred must have distinguished himself in war, though, as in the case of the last Montezuma, he was a member of the priesthood. This singular mode of supplying the throne had some advantages. . . . . The scheme of election, however defective, argues a more refined and calculating policy than was to have been expected from a barbarous nation." This account is very brief and general, but it so happens that the truthfulness of this law of descent is verified in both particulars. Montezuma was succeeded by his brother Cuitlahua; and upon his decease, a few months afterwards, he was succeeded by his nephew Guatemozin, also a nephew of Montezuma.

Had the researches of this elegant writer brought him in contact with the real institutions of the Aztecs which controlled this question of descent, he would have discovered, there is every reason to believe, that the people were divided into tribes, with laws of descent precisely similar to those of the Iroquois. That no Aztec could marry into his own tribe; that the children were of the tribe of the mother; and that all titles were hereditary in the tribe, and descended in the female line. This also furnishes a sufficient reason why Montezuma was succeeded by his brother, and the latter by his nephew; and why it was impossible for a son to succeed his father.*

In a note (I. 39, n.), Mr. Prescott adverts to the existence of tribal divisions of the people, as follows: "The people of the provinces were distributed into calpulli, or tribes, who held the lands of the neighborhood in common. Officers of their own appointment parcelled out these lands among the several families of the calpulli, and on the extinction or removal of a family, its lands reverted to the common

* The tribal device of Montezuma was an "Eagle," from which it is to be inferred that he belonged to the "Eagle tribe," and that the sachemship held by him was hereditary in that tribe.

stock, to be again distributed. The individual proprietor had no power to alienate them. The laws regulating these matters were very precise, and had existed since the occupation of the country by the Aztecs." The land system of the Iroquois was much the same, but with this addition, which was probably true also of the Aztec, that an individual could sell his improvements; and as the fee of the land belonged to the people in common, it was practically equivalent to owning the fee. The Indian races of this continent have always held their lands by this simple tenure. Any one can occupy vacant land, and improve it; and this act makes it private property. When abandoned, it reverts to the common stock, and is open to the next occupant. If he sells his improvements, as he may, the purchaser is protected in his enjoyment. The note above referred to contains all that Mr. Prescott thought worthy of mention concerning the tribes of the Aztecs; and he does not appear to have recognized their tribal organization as a primary institution of any importance.

It is well known that the early Spanish writers, upon the conquest, are full of contradictory assertions, exaggerations, and fabulous statements. Very little reliance is to be placed upon them. On the other hand, the institutions of our Indian races are obscure and complicated; and can only be worked out by careful and patient research, carried down to minute particulars. Those of the Iroquois were unknown, until within the last twenty years, although the Jesuit missionaries, and both French and English travellers, had written volumes upon their civil and domestic affairs. The real structure and principles of the league eluded their inquiries. Its general features were well known, but were so incumbered by errors that the knowledge was of but little value. It would not be surprising if the same were true of the institutions of the Aztecs.

Now the institutions of all of the aboriginal races of this continent have a family cast. They bear internal evidence of a common paternity, and point to a common origin, but remote, both as to time and place. That they all sprang from a common mind, and in their progressive development have still retained the impress of original elements, is abundantly verified. The Aztecs were thoroughly and essentially Indian. We have glimpses here and there at original institutions which suggest at once, by their similarity, kindred ones among the Iroquois and other Indian races of the present day. Their intellectual characteristics, and the predominant features of their social condition, are such as to leave no doubt upon this question; and we believe the results of modern research, upon this point, concur with this conclusion. Differences existed, it is true, but they were not radical.. The Aztec civilization simply exhibited a more advanced development of those primary ideas of civil and social life, which were common to the whole Indian family, and not their overthrow by the substitution of antagonistic institutions.

Judged, then, from the institutional point of view, the Aztec monarchy, as described to us by current histories, will not bear the test of criticism. So far as the structure of the government is concerned, a serious doubt rests upon the whole narrative. The testimony, drawn from the very nature of true Indian institutions, denies that the Aztec government was a monarchy. Nay, it asserts that it is utterly impossible that it could have been a monarchical government. Venturesome as this statement may appear, it is yet proclaimed and vindicated by the principles and structure of Indian society. If we could now break through the overlying mass of fable and exaggeration, and bring to light the real institutions of the Aztecs, it would be found, there is every reason to believe, that their government was an hereditary oligarchy, very similar to that of the Iroquois. That Montezuma, so far from being emperor of the Aztecs, was one of a large number of sachems, who, equally, by their joint authority in council, administered the affairs of the commonwealth. That, as the leading sachem residing in the metropolitan city, he was first brought in contact with the Spaniards; and they, having taken it for granted that he was the emperor, determined that he should be emperor, right or wrong. The splendor and power of the Aztec monarch, as set forth in their recitals, tended, in no inconsiderable degree, to magnify their exploits.

The evidence which supports this position is drawn from the nature of their institutions, so far as they are known, from the structure of Indian society, and from the general principles of action which distinguish the hunter state. The necessary limits of this article will not admit of the presentation of this evidence here. Attention, however, may be called to a single fact stated by Mr. Prescott. "According to some writers of authority," he says, "there were thirty great *caciques*, who had their residence, at least a portion of the year, in the capital, and who could muster a hundred thousand vassals each, on their estates. Without relying on such wild statements, it is clear from the testimony of the conquerors, that the country was occupied by numerous powerful chieftains, who lived like independent princes on their domains" (I. 26).

This division of generic races into tribes, with descent in the female line, has prevailed very generally throughout the Indian family. Besides the Iroquois, and with every probability the Aztecs, we may mention that the Creeks, were divided into ten tribes; the Chickasaws into six; the Ojibeways into thirteen; and the Delawares into three. As the last two were the principal branches of the Algonquin family, it is reasonable to infer that similar laws of descent prevailed throughout all of its subdivisions.

Neither was it confined to this continent, but is found, at the present time, on some of the islands of the Pacific. In 1852, missionary stations were established among the Micronesian islands. One of them, Ascension island, belonging to the Kingsmill group, was occupied by the Rev. Dr. Gulick. He writes to the Board as follows: "The present population (of this island) is divided into five hostile tribes, each of which is divided into seven or eight independent clans" (Miss. Her., May, 1855, p. 131); or, as we should say, there were five nations, and each nation was divided into seven or eight tribes. In like manner, the Rev. Mr. Snow, an associate missionary, but located upon Strong's island, writes, in relation to the succession of chiefs, that by a singular provision the office descended to a nephew of the deceased chief, and not to his son. In a previous report, Dr. Gulick, referring to the oldest European resident upon the island, and who had married a native woman, says, "A former wife of Mr. Corgat was a high chief, and his children, now living, are high chiefs in the tribe, for *rank descends through the wife*" (Miss. Her., 1853, p. 90). The writer has been informed, but cannot verify the fact at the present moment, that similar laws of descent prevail among the native tribes of Australia, and also of New Grenada, in South America.

Whether this code of descent came out of Asia, or originated upon this continent, is one of the questions incapable of proof; and it must rest, for its solution, upon the weight of evidence, or upon probable induction. Its existence among American races, whose languages are radically different, and without any traditional knowledge among them of its origin, indicates a very ancient introduction; and would seem to point to Asia as the birth-place of the system. The writer has no means of ascertaining whether similar laws of descent existed among the Tartar races. It is to the Tartar branch of the Mongolian family that attention would first be excited, from the pertinacity with which some of them have adhered to the nomadic state, and from the similarity of domestic habits to be found in the hunter life. Among the classic races we should hardly expect to find any similar institution, and yet in the account given of the Lycians, of Asia-Minor, by Herodotus (Herod. Lib. 1, c. 173), we have a glimpse at a system of descent, which, as far as it is explained, is analogous to that of the Iroquois. He says, "Their customs are partly Cretan, and partly Carian ; but they have one peculiar to themselves, in which they differ from all other nations, for they take their name from their mothers and not from their fathers ; so that if any one ask another who he is, he will describe himself by his mother's side, and reckon up his ancestry in the female line. And if a free-born woman marry a slave, the children are accounted of pure birth ; but if a man, who is a citizen, even though of high rank, marry a foreigner, the children are infamous."

Before dismissing this subject, another question remains to be considered. For what particular end was this code of descent established? What great object was sought by making the title of chief or sachem hereditary in the female line, thereby perpetually disinheriting the son? Several reasons may be assigned without, perhaps, giving the real one. It is quite apparent that it secured the purity of descent by a rule infallible ; for the sachem was necessarily the son of his mother, although he might not be the son of his mother's husband. Again, by assigning one or more sachemships to a tribe, the individuality of the tribe was maintained ; and thus the unity and integrity of Indian society, in its primary organization, were preserved. But neither of these sufficiently account for it. There is another view of the matter which may, perhaps, come nearer the truth. The hunter state is essentially one of individual freedom ; and this freedom, as well as the mode of life to which it was incident, became a passion to which the Indian mind was attuned from its lowest depths. He desired, and would tolerate, no other state. Under this tribal organization, generic stocks might divide and subdivide indefinitely, until they spread over a continent, and still preserve the integrity of the hunter state. They could also unite their tribes into nations, and the nations into a league, for mutual protection. It mattered not how far they spread apart, by emigration, from the original centre, a national unity would be maintained until they were separated, geographically, by intervening races. Now it is evident that this tribal organization, with the descent of the office of sachem in the female line, and limited to the tribe, was eminently adapted to the hunter state; and, secondly, that it was preëminently suited to the formation of confederacies of kindred tribes. The original tribe divided into two, and subsequently, as their numbers increased, the two subdivided into four, and perhaps again into eight, creating new sachemships with each subdivision, and spreading abroad over a larger area. They were still of common origin, bound together by the ties of consanguinity, and the several tribes intermingled through and through by intermarriage. Although, by the lapse of time, they had become scattered over a large territory, all the necessary elements of federation existed in the very structure of their society. Hence it was perfectly simple and natural to retrace the steps by which they had been weakened through subdivision, and seek for its obvious correction in a league of tribes. This is, in reality, a condensed statement of the actual history of our Indian races. They have undergone a process of repeated and continuous subdivisions from age to age, but counteracted here and there by confederacies. We know that these confederacies have existed, and still exist, in places, all over this continent; as witness, among others, the league of the Iroquois, the Powhattan confederacy, in Virginia, the Sioux league of the Seven Council fires, and the alliance between the Aztecs, Tezcucans, and Tlacopans. But, on the other hand, we have never known of an Indian monarchy on any part of it, unless we accept the pretended Aztec monarchy. By the junction of several tribes into one nation, and several nations into a confederacy, the people are brought under the joint authority of the sachems of the several tribes, who, in general council, administer all such affairs as relate to the common welfare, leaving each tribe and nation to the particular government of its own sachems. Such a government was that of the Iroquois; and substantially, without much doubt, the form of government which prevailed among the whole Indian family upon this continent.

But, on the other hand, break down this barrier of descent in the female line, and allow the sachem to perpetuate his power in his own family, and it is easy to see that a consolidation of tribes under one sachem would be the result; and this process of consolidation would progress by conquest, until despotism would succeed to freedom, and the hunter state would be overthrown.

This tendency was arrested at the fountainhead, by two simple provisions. The first, was the disinheritance of the son by their laws of descent; and the second, was the total separation of the military from the civil power. This last fact has been elsewhere adverted to; but its importance has not been made sufficiently prominent. Under the Indian theory of government, the sachem was a civil ruler, whose duties were confined to home or peace affairs. He was not prohibited from going out upon the war-path, but before he could do so, he was compelled to lay aside his office. The Indian war system was purely a voluntary one. Theoretically, every nation was at war with every other nation with whom it had not formed an actual alliance. Any one, but a sachem, could enlist a band, and go out upon his own responsibility, and he thus become the captain, by the compact of enlistment, for this single enterprise only, of those who agreed to follow him. When two or more bands united upon a common expedition, each one continued under its own captain, and all joint military measures were agreed upon by the war captains in council, as they rarely ever appointed a commander-in-chief. When the Iroquois sachems, in general council, declared war against any other nation, no levies were made, or public measures adopted for its prosecution, but it was left to private enterprise and the sense of patriotism; and the war itself was conducted by volunteers; by volunteer captains, as well as warriors. Now it is easy to see what a powerful influence these two limitations, upon the authority and influence of their sachems, exercised for the perpetuation of the hunter state. Whether they are to be placed, with reference to this state, in the relation of *cause*, or of *effect*, they go far to account for the otherwise extraordinary phenomena of Indian life.

From these limitations, the office of sachem never conferred any great degree of authority or influence; neither was it favorable for the development of talent. The war captains and orators were usually far more influential, among the people, than their rulers. We have a singular proof of this fact in the sachems of the Iroquois. Although there were fifty of them at all times in office,* and this number must be multiplied by as many generations as have passed away since the league was formed, yet not one of this long list of sachems is particularly distinguished in their own traditions, except the first To-do-dä'-ho, and the first Da-gä-no-wé-dä, who were the founders of the league; and not one of them has left a name among us, except Logan, who, under his true Indian name, was one of the ten Cayuga sachems, and Gä-ne-o-di'-yo, one of the Seneca sachems, better known as Handsome Lake. All of their distinguished men belonged to the class of chiefs, who were elected such in reward of merit; among the number may be mentioned Red Jacket (Sä-go-ye-wät'-hä), Brant (Tä-yen-dä-nae'-ga), Gar-ran-gu-la, Schenandoah (Sken-an'-do), Cornplanter (Gy-ant'-wä-ga), Steeltrap (Kar-is-ta-gi-a), and Big Kettle (So-nó-jo-waw-ga).

The American Indians must have discovered, by experience, that in the precise ratio in which they increased the power of their chiefs, they surrendered their personal freedom; and as their history upon this continent bears continuous and overwhelming testimony to their enthusiastic devotion to the hunter life, it is not unreasonable to conclude, that they cherished such institutions as would perpetuate this state in its integrity, and resolutely opposed such innovations as would endanger the freedom it bestowed.

C. PRACTICAL SCIENCE.

1. Views and Suggestions on the Practice and Theory of Scientific Publication. By Lieut. E. B. Hunt, Corps of Engineers, U. S. A.

It is much to be regretted that physical investigators labor under so many and serious disadvantages for the successful prosecution of their

* There were, in reality, but forty-eight. Two of the Mohawk sachemships were left vacant after the demise of the first sachems who held the titles. They were Hä-yo-went-hä and Da-gä-no-wé-dä. Tradition says, they were accepted on these terms, which were imposed by the sachems themselves.

researches. In addition to such hinderances as result from the stern necessity of procuring a livelihood, there are many which do not seem necessary, but which still effectually obstruct that progress towards new truth, by which the man of science measures success. Not least among these adverse circumstances is the almost universal fact, that the man of science has not the command of those books and special memoirs, in which are recorded the methods and results of anterior investigations. Except in the rare cases of conjunction between science and wealth, there is a perpetual inability to compare the research in hand with the great body of related researches of the past and present times. It is scarcely possible that one's theme should be entirely his own property, and, except in this rare case, it is very needful that he should have free and habitual access to all records bearing on his particular line of research. Now this is utterly impossible, unless the investigator lives in convenient proximity to some one of the principal libraries. There is not yet, in this country, any single library at all equal to the entire demands of our whole body of investigators, and the two or three which are slowly approximating to this position of scientific catholicity, are only very partially available beyond the charmed circle of "an easy walk." In making a birds-eye examination of from six to seven thousand volumes of scientific memoirs, periodicals, etc., while gathering materials for an index of references, to be published by the Coast Survey, I have been more and more impressed with the vast affluence of the published records of science, and of the capacity of the many thousands of memoirs and articles passed in review for a strict classification under specialities. It is entirely a safe assertion, that no specialist, however restricted his speciality, is fully cognizant of all the published matter relating to his field; and also, that without this full knowledge of labor and thought already expended, he must be wasting much of his own strength in doing again what others have completed. In point of fact, most of the active, truth-seeking minds of this country are compelled to work on, remote from the chief central depositories of books and memoirs, happy if from their limited private resources they can secure, as a private possession, that carefully chosen minimum of special treatises which will give a starting point somewhere near the front line of actual research or discovery. Nearly all such isolated effort is expended too much at venture to bear the fruit of discovery to which it ought and is entitled to aspire. Not one in twenty of the members of this Association has the command of all those special records of research which are needful to the symmetry and completeness of his own labors. There may be, in the Astor Library, a dozen closely related memoirs of which a specialist knows the existence, and a dozen more of which he has never heard; but if he must journey some thousands or hundreds of miles, at his own cost, to study them, from 10 A. M. to 5 P. M., he is fortunate if he can use them on these conditions. The result is, that only a few amongst us are able to do as we should, had we a complete library within easy reach. Such is nearly the universal truth concerning those who are by nature endowed with the faculties requisite for successful research. It cannot, therefore, be entirely profitless to consider what possible alleviations of this condition are practicable.

First, then, it is a fixed tendency of scientific research to subdivide, during its progress, into numerous and well defined specialities. Each new branch of investigation soon subdivides into other and more limited branches. Though all are ultimately derived from one grand trunk, so vast that not even the giants of mind can span its circumference, the special branches have a narrowness of diffusion, which brings them quite within the compass of a single scientific mind. It thus happens that though many thousands of separate memoirs or investigations are preserved in the aggregate records of science, the pure specialist will find but few which are related to his own chosen field; so few, indeed, that, could they be bought separately at the ordinary prices for printed matter, he might, without harming his fortunes, possess as his own the essential materials for a complete study of his speciality. If, however, he attempts this solution of his wants, he is at once confronted by the fact, that this small group of memoirs cannot possibly be purchased without buying in connection the very costly series to which they severally belong. For the most part, the series of scientific memoirs is grouped together, without reference to subject, in a most illogical congeries, having only the date and the publishing society in common. Hence the specialist, when outside the limits of easy access to a complete library, must rely almost entirely on separately published special treatises for his knowledge of what has been done, and the original memoirs of the master-minds must be forbidden treasures.

It is, however, obvious that such a lame condition of things is not one of original necessity; it has grown out of the history of scientific publication. If we go back to the times preceding the existence of academies of science, we find nothing answering to the scientific memoir. The academies formed from those who were rich in ideas, but poor in purse, first found that, by associating the limited wealth of a number, the great result, otherwise denied, could be attained. When, too, they assumed an imposing exterior, through the lustre of great names, the condescensions of patronage intervened, and the luxuries of type and engraving began to be lavished on the volumes in which their proceedings and choice memoirs were published. As science expanded its domains, and augmented its small array of followers, academies sprang up in profusion and in ever increasing numbers, until now they have outrun the knowledge of even the most learned librarians. Nearly all of them have published, or now publish, volumes of memoirs, which are the tokens of their vitality to all not within the chosen band. Wherever strong discovering minds have elaborated their researches, the volumes of memoirs are enriched with treasures of close reasoning, sagacious insight, and results laboriously attained. These memoirs are still fresh with the vigor of original thinking, and cannot be spared, as part of the training of future discovering minds. When original memoirs give place, in the studies of investigators, to bald abstracts, feeble analyses, or popularized digestions, these investigators will grow sinewless, and discoveries, if made at all, will come despoiled of all their proper grandeur. We cannot too strongly deprecate the tendency, even among specialists, to know the labors and investigations of original scientists only through abstracts and synopses or general reports. These are indispensable aids for the knowledge of the immense related area outside of one's speciality, which he can only expect to know at second-hand, and by the briefest method. But they are not among the legitimate means of advancing the boundaries of his own special field. Direct resort to original records is the prime condition on which the symmetry and well furnished culture of specialists must rest.

In the true and philosophical sense of the word, most of the printed scientific memoirs cannot be said to have been published. Publication implies something more than mere printing. It involves the idea of circulation or diffusion among the whole group of kindred minds specially interested in the particular subject-matter. A memoir on electro-magnetism, for instance, is not published when it is printed in the

St. Petersburg Bulletin, because very few of those specially prosecuting electro-magnetism habitually see this Bulletin, and single copies are not readily obtained. It is possible that nearly all of this class will somewhere see a bald announcement, that Lenz has, after many experiments, come to such and such conclusions; but this leaves them almost as much in the dark as before. Each memoir has its special public, diffused over all lands where science is cultivated, and is fully published only where it has reached the whole of this public. In some instances this special public would number less than a hundred; in some others, it would be many thousands. To all not members of its own proper public, any special memoir is as an utterance of unknown tongues, and however much they may reverence, they cannot duly appreciate it. If, therefore, a society or academy has procured the printing of a valuable memoir, it has the great matter of publication still left to attend to, so long as those specialists throughout the world, for whom the memoir is really meant, are not duly supplied with printed copies. A memoir may nearly as well not exist, as be printed and not properly circulated. As a matter of fact, a large proportion of the most valuable printed memoirs have never been published at all. There is enough science buried in the documentary files of our general and state governments to fill a long series of volumes. These scientific waifs are scattered broadcast among politicians and their constituents, but the humble devotee of science, who spends years of labor in kindred researches, vainly strives to possess himself of a copy. In such cases, myriads of copies may be printed, and yet the memoirs are not published. On the other hand, if there be a special memoir on a subject which only ten men have studied or will study, the supplying each of these ten with a copy is a complete publication.

A great portion of the failure to publish printed memoirs is clearly consequent on the mode of publication. Nearly every academy prints its own proceedings and memoirs, aided by citizens whose local pride is enlisted in the promise of scientific distinction. With the printing, however, the matter is prone to end, there being no effective mode of publication. Scientific periodicals, in like manner, though far more generally published than academic memoirs, are still limited to a small and inadequate circle of subscribers, and beyond this circle they are in effect not published.

It is evident, on reflection, that the great desideratum for the correction of the evil indicated, is a *universal publishing agency* for all scientific memoirs printed by societies and academies, and for the periodicals of the entire scientific world. Without at all interfering with the formation of regular library files by society donation or purchase, as is now done, such an agency would have enough to do in supplying all specialists with their special memoirs. The expansion of this idea into practical form would, of course, rest on the concurrence of all the societies, etc., which publish memoirs. I can, perhaps, most clearly indicate its workings by supposing the agency established; its system might be, briefly, this: —

An agency is duly organized and guarantied to the world, by being placed under the control of a board or well-established institution or society. The agent puts himself in correspondence with all the societies which publish memoirs, and, after fully explaining the objects of the agency, makes arrangements with each society to print a given number of separate copies of each memoir issued, making what might be called the specialists' edition. The society might either furnish these copies at the cost of manufacture, or at a fixed price, or, if it is able, it might donate them. Thus the universal agent will be supplied with unbound copies of each memoir. He is all this while supposed to be corresponding with all the specialists, for the purpose of ascertaining what class of memoirs each one may desire. He will soon be able to supply printed lists of titles, whereby each investigator can know what memoirs just published or announced as forthcoming he may desire. Thus a perfect understanding of the relation between supply and demand might be attained. This being all done, and the agency fully supplied, Prof. A. B., living anywhere, orders a given number of particular memoirs, at prices indicated in the list. He has only a limited surplus of means to bestow, but he can thus supply his exact wants at the minimum cost, and without having to buy a large number of memoirs he does not want. The result will be, that Prof. A. B., in a town remote from large libraries, can proceed in his chosen studies, with all the aids needful for placing himself in advance of the known, and thus becoming an effective discoverer. The main object would be not profit to the agency, but simply to give demand a full opportunity to call upon supply. As far as it is now possible, without reprints, the memoirs hitherto published would be included in the range of operations. An author publishing a special memoir in a periodical, by sending copies to such an agency, would be sure of their going to the right persons. It is likely that many economical reprints might in time be justified. The main idea, however, would be to insure the proper publication of new memoirs as they are printed. Such an agency ought not to be an ordinary commercial agency without guaranty of its continuance, but it should rest on a firm basis, so as everywhere to deserve and possess confidence.

There are laws of political economy underlying the philosophy of publication, which have been too little regarded in scientific issues. They have mostly been published on the plan of large prices and small circulation. The right plan is a maximum circulation and a minimum paying price. There are enough ready to buy almost any special memoir to authorize its being sold at a low price. If, however, a high price is affixed, it becomes at once prohibitory. Such publications as Taylor's Scientific Memoirs, and Liebig and Kopp's Reports, ought not to be expected to succeed at such prohibitory prices as they bear. All that is necessary to insure success to the principle of minimum paying prices and maximum circulation is simply to organize the demands of scientists throughout the world, so as to insure to such specialist a chance of purchasing precisely what he wants at fair rates. By doing this, scientific progress will be immensely stimulated, and the poor barriers of cliques, nations, languages, and races will be swept away before the widely diffused liberalities of truly cosmopolite science. If the imperfect glimpses towards some vital generalities which I have now essayed shall lead to a more correct appreciation of existing evils, and to active reflection on the possible meliorations in the condition of the scientific body, my purpose will be fulfilled.

TITLES

OF

COMMUNICATIONS.*

A. MATHEMATICS AND PHYSICS.

1. NOTE ON THE CONSERVATION OF FORCE. By BENJAMIN PEIRCE.
2. MEMOIR ON THE MOLECULAR CONSTITUTION OF MATERIAL MASSES. By E. B. HUNT, Lieut. U. S. Engineers.
3. NOTE ON THE GYROSCOPE. By BENJAMIN PEIRCE.
4. SOME FURTHER STATISTICS AND INQUIRIES WITH RESPECT TO THE FORM, MAGNITUDES, ETC., OF THE ASTEROID PLANETS. By STEPHEN ALEXANDER.
5. ON THE RESEMBLANCE BETWEEN THE ARRANGEMENTS OF THE SOLAR SYSTEM AND THE PHYLLOTAXIS IN PLANTS. By STEPHEN ALEXANDER.
6. ON THE SPECIAL HARMONIES OF THE DISTANCES AND THE PERIODIC TIMES OF THE BODIES COMPOSING THE SOLAR SYSTEM, AND THE PHYSICAL HYPOTHESIS TO WHICH THIS STATE OF THINGS SEEMS TO BE REFERABLE. By STEPHEN ALEXANDER.
7. ON SOME SPECIAL RELATIONS OF THE STRAIGHT LINE, AND THE VARIOUS ORDERS OF CURVES. By STEPHEN ALEXANDER.

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* The following papers were also read, and would have been printed in full, if a copy of them had been furnished for publication. It was ordered by the Standing Committee, in conformity with the practice of several years, that "no notice of articles *not approved* shall be taken in the published Proceedings."

8. On the Diverse Weights employed in Modern Coinage. Part I. On Troy Weights. Part II. On Karat Grain Weights. Part III. On Avoirdupois Weights. Part IV. On the Metrical System of France. By J. H. Gibbon.

9. On a Peculiar Form of the Fiducial Barometer. By J. Constantine Adamson.

10. An Inquiry into the Causes of a Peculiar Discoloration of Coffee, which had been Stored in the same Building with Guano. By Dr. Lewis H. Steiner.

11. A Note on Arithmetical Complements. By Thomas Hill.

12. Sketch of a Plan proposed for reducing Observations upon Periodical Phenomena to a Series of Mean Dates, and the Advantages of this Method in Developing the Laws of Climate. By Franklin B. Hough.

13. A Critical Review of the different Measurements of Mount Washington, in the White Mountains of New Hampshire. By Prof. Arnold Guyot.

14. On a Series of New Meteorological and Physical Tables prepared for the Smithsonian Institution. By Prof. Arnold Guyot.

15. On the Barometric Formulæ for the Measurement of Differences of Level by the Barometer, and a proposed Modification of Laplace's Constant. By Prof. Arnold Guyot.

16. On a New Maximum Thermometer. By Thomas Green.

17. On a New Method of Determining the Commercial Value of Saltpetre. By E. N. Horsford.

18. On the Painting of Pictures with Both Hands. By E. N. Horsford.

19. On Surety Paper. By E. N. Horsford.

20. On some Phenomena of Ice. By Prof. Joseph Henry.

21. On the Physical Conditions determinate of the Climate of the United States. By Prof. Joseph Henry.

22. Observations on the Zodiacal Light at Quito, Equador, with Deductions. By Rev. Geo. Jones, U. S. N.

23. Some of the Phenomena of the Texas "Norther" and Climatology. By G. C. Forshey.

24. On an improved Construction of Ruhmkorff's Induction Apparatus. By E. S. Ritchie.

25. Improvement in the Construction of the Achromatic Telescope. By H. L. Smith, Kenyon Coll., Gambier, O.

26. On some Modes of preventing the Counterfeiting of Bank-Notes. By T. Sterry Hunt.

27. A Description of several new Ternary Compounds — Chromites. By A. K. Eaton.

28. On Bank-Notes, and the Means adopted for Prevention of Counterfeiting. By Benjamin Silliman, Jr.

29. Notice of a New System of Dressing Metallic Ores. By Benjamin Silliman, Jr.

30. Solar Eclipse of March 14–15, 1858. By Thomas Hill.

B. NATURAL HISTORY.

31. On the Age and Dip of the Connecticut River Sandstones, and the Intercalation of the Associated Trap. By Edward Hitchcock.

32. Contributions to the Geology of California and the Gadsden Purchase. By T. Antisell.

33. On the Origin of Anthracite and Bituminous Coal. By Charles Whittlesey.

34. Note on the Formation of Continents. By Benjamin Peirce.

35. On the Physical Structure of the Continent of Africa. By Arnold Guyot.

36. On the Cretaceous Formations of the United States and the North American Continent. By James Hall.

37. On the Carboniferous Limestones and Coal-Measures of the United States. By James Hall.

38. On the Direction of Ancient Currents of Deposition, and the Source of Materials in the older Paleozoic Rocks, with Remarks on the Origin of the Appalachian Chain of Mountains. By James Hall.

39. On the General Plan and Mode of conducting the Geological Survey of Great Britain. By A. C. Ramsay.

40. On the Physical Break and the Break in the Succession of Life in the British Rocks. By A. C. Ramsay.

41. On the Occurrence of Sharks' Teeth in the Drift of the Mississippi. By A. H. Worthen.

42. On the Parallelism between the Rocks of the Laurentian and Silurian Systems. By T. Sterry Hunt.

43. General Considerations on the Metamorphism of the Sedimentary Rocks. By T. Sterry Hunt.

44. On some Mineral Waters, and on the Origin of Magnesian Rocks. By T. Sterry Hunt.

45. On a Subsidence of the Coast of New Jersey, and of some of the adjoining States. By Geo. H. Cook.

46. On a Species of Trilobite from the Potsdam Sandstone of New York. By J. D. Dana.

47. On the Flexures of the Strata in the Broadtop Coal Field, Pennsylvania. By J. P. Leslie.

48. On the Parthenogenesis of Animals and Plants. By Berthold Seemann.

49. Remarks on the Classification of the Varieties of Man. By M. B. Anderson.

50. Remarks on the Collection of Indian Paintings and Antiquities exhibited by Mr. Paul Kane, at the Request of the Local Committee. By Daniel Wilson.

51. Observations on the Manners and Customs of the Indians of the North-west; illustrated by Paintings executed by the Author during his Travels. By Paul Kane.

52. On some Ethnological Specimens from the Island of Aneiteum, New Hebrides. By J. W. Dawson.

53. On certain Peculiarities of the Languages of Southern Africa. By J. Constantine Adamson.

54. On some Debatable Points in Natural Science. By E J. Chapman.

EXECUTIVE PROCEEDINGS

OF THE

MONTREAL MEETING, 1857.

HISTORY OF THE MEETING.

THE Eleventh Meeting of the American Association for the Advancement of Science was held at Montreal, Canada East, commencing on Wednesday, August 12, and continuing to Thursday noon, August 20.

The number of names registered in the book of members in attendance on this meeting is three hundred and fifty-one. One hundred and fifty-nine new members were chosen, of whom all but forty-one have already accepted their appointment by paying the assessment, and eighty-six of them signing the constitution. One hundred and seventeen others joined the Association by virtue of Rule 2 or 3, all of them signing the constitution, and paying the assessment. Nineteen others paid, without a formal election, or signing the constitution; and sixteen signed the constitution without a formal election, and without paying. One hundred and thirty-two papers were presented, of which one hundred and ten were read; but only a part have been printed. Some were thought unworthy of publication, and, in other cases, copies have not been furnished by the authors.

The sessions of the Association were held in the Court House.

The late President, Professor James Hall, of Albany, introduced to the Association the Vice-President for the meeting, Professor Alexis Caswell, of Providence. The Vice-President announced that the meeting would be opened by a prayer from the Lord Bishop of Montreal, Rev. Francis Fulford. The following prayer was then offered:—

"O Lord our God, who by thy Almighty word hast created the world, and now upholdest and governest all things in heaven and earth, receive, we beseech thee, these our humble prayers, which we offer to thy divine Majesty; and as thou didst at the first teach the hearts of thy faithful people by sending to them the light of thy Holy Spirit, and hast graciously promised to continue his presence with them that ask for it, grant us by the same spirit to have a right judgment in all things; and specially direct and guide the deliberations and actions of this Association, in connection with which we are assembled here this day. May all our works be begun, continued, and ended in thee; and while in the eager pursuit of knowledge and the deep investigations of science, may we be led to exclaim that 'The heavens declare the *glory* of God,' let us never rest satisfied, O Lord God Almighty, unless from thy Word, which thou hast given us, we can also learn *thy will*, and grow in grace, as well as knowledge, and in meetness for enjoying thy presence afterwards, when we may hope to see thee face to face, and know even as we are known. Let neither the splendor of any thing that is great, nor the conceit of any thing that is good in us, withdraw our eyes from looking upon ourselves as sinful dust and ashes. And thus clothed with humility, may we be meek, charitable, and courteous towards each other, so that no evil spirit of anger, jealousy, or vainglory, may find place in the midst of us to hinder or disturb our unity and peace.

"These petitions we offer up in the all-prevailing name of Jesus Christ, our only Mediator and Redeemer, with whose most perfect form of words we conclude our imperfect address to the Throne of Grace.

"'Our Father which art in heaven, hallowed be thy name, thy kingdom come, thy will be done on earth as it is in heaven: give us this day our daily bread, and forgive us our trespasses, as we forgive them that trespass against us; and lead us not into temptation, but deliver us from evil; for thine is the kingdom, and the power, and the glory, for ever and ever. Amen.'"

Then the Vice-President made the following address:—

GENTLEMEN OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE:—I congratulate you upon so large an attendance of members at the opening of this your Eleventh Annual Meeting. It augurs well for the interests of science, that so many of her votaries from distant portions of the country have come up to this gathering to lay their choicest contributions upon her altar, and welcome to their fellowship the humblest laborer in her cause.

I think it is also a matter of special interest that we are convened without the limits of the United States. However it may have been in former times, it certainly is not now the case, that mountains or seas interposed make enemies of nations. It is one of the felicities of our times, that, in the onward march of science, little account is taken of the boundaries which separate states and kingdoms. The discoverer of a new law in nature, the inventor of a new process in the arts, or a new instrument of research, is speedily heralded over land and ocean, is put in communication with the whole civilized world, and is everywhere hailed as a benefactor.

We have before us an illustration of the amenities of science. We of the United States are here assembled on British soil, little thinking that we have passed beyond the protection of American law,—little thinking, amid the generous hospitalities of this enterprising commercial capital of a noble province of the British Empire, that we are alien to the British Constitution. The American Eagle has left us, but I assure the gentlemen who have invited us here, that we feel in no danger of being harmed by the British Lion.

I have said that we are alien to the English Constitution. This, of course, is true only in the narrowest and most technical sense. I am proud to say, as I do upon deliberate conviction, that nothing is alien to the English Constitution which looks to the advancement of knowledge, the perfection of art, or the amelioration of the condition of humanity. I may further say,—and I here speak by permission of his Excellency, the Director of the Government,* who honors us by presiding at this meeting,—the proudest achievements of British arms have less enduring glory than that patronage of science, and those no-

* General Sir William Eyre.

ble efforts for the advancement of Christian civilization, which, no less than her commercial greatness, place old England in the van of nations. And never, I may add, has that patronage been more efficient, or those efforts more praiseworthy, than under the reign of the present illustrious Queen, whose virtues are alike the ornament of her sex and her crown.

There is seemingly a special fitness in our being here at this time. England and America are at this moment preparing to shake hands across the broad bosom of the Atlantic. The electric chain which is to bind them in perpetual friendship has been fabricated, and is now being deposited along the deep bed of the ocean, where no surface storms can disturb its repose or impair its energy. Thus United England and America may challenge the world, not, indeed, to the conflict of arms, but to the beneficent rivalry of extending the domain of science, of unfolding the wonderful mechanism of the heavens and the earth, and of rendering the powers of nature subservient to the uses of man.

But our congratulations on this occasion are not unmingled with sadness. It is a melancholy event which calls me to preside over your deliberations. The distinguished gentleman whom, at your last meeting, you elected to the presidency of this Association, is no longer numbered with the living.

Jacob Whitman Bailey, Professor of Chemistry, Mineralogy, and Geology in the U. S. Military Academy at West Point, died, after a protracted illness, on the 26th of February last, in the 46th year of his age. It is to be hoped that before the close of this meeting a fitting tribute will be paid to his memory, and to his eminent service in scientific research.

As a resident in the city where his youth was spent, I will simply say, that he gave early indications of that love of retired study and investigation which became so conspicuous in his maturer years. Many of his youthful companions remember the importance which he was accustomed to attach to every new variety of flower or pebble or shell which came in his way. At the age of seventeen he received an appointment as a cadet in the Military Academy at West Point. Though but a youth, and slender in health, he accomplished the severe course of academic studies with entire success. He held the honorable rank of the fifth in a class graduating forty-five members. He left the institution in 1832. In 1834 he was called to give instruction in the department of Chemistry, Mineralogy, and Geology. In 1838 he received the full appointment of Professor, and was the first to fill that chair. For more than eighteen years, says the official order announcing his death, he discharged the duties of that post with signal ability and success. In addition to the branches of science formally embraced in his professorship, he was well versed in Natural History, and specially in Botany. But it was doubtless in his extended microscopical researches that he has most contributed to the stock of human knowledge, and laid the foundation for the most enduring fame.

Amid the endearments of family and home, which ever awakened the finest sensibilities of his nature, he was called to drink deeply of the cup of affliction. His wife and daughter perished by the burning of a steamboat on the Hudson River on the 28th of July, 1852. He is believed never fully to have recovered from the shock of that heart-rending event.

Mine would not be the task, nor is this the place, to enter upon any analysis of his scientific labors. It is sufficient to say, that he established a reputation that does honor to American science, and places him among the most distinguished microscopists of the age.

In discharging the duties, gentlemen, of your presiding officer, I must bespeak your indulgence and your friendly coöperation. The courtesies which you are accustomed to show to each other will compensate for any inexperience on my part, or any want of knowledge in parliamentary forms and usages. We are met together with no narrow and selfish aims. Our object is to advance, so far as we may, the great cause of science, which embraces within its scope the great interests of humanity. In this noble enterprise, let us tender to each other every aid and every encouragement in our power.

At the conclusion of the President's address, Sir William E. Logan, in behalf of the Local Committee and the citizens of Montreal, welcomed the members of the Association in a few words:—

MR. PRESIDENT AND GENTLEMEN OF THE ASSOCIATION:—The welcome which I could give could only be a welcome to Montreal; but His Excellency, the Head of the Government, recognizing the importance of this meeting, and the interest attached to it, has kindly undertaken to give you a welcome, which will not be merely on the part of the citizens of Montreal, but on the part of the whole of Canada; and not merely of the whole of Canada, but on that of all the British North American possessions. I shall therefore leave it to His Excellency to give you that welcome.

His Excellency, the Administrator of the Government, General Sir William Eyre, K. C. B., next rose and said: —

LADIES AND GENTLEMEN: — It is not my desire or intention to detain you more than a few moments, as I have no doubt you will be anxious to commence the business of the meeting. But I wish to express, on behalf of Canada and myself, a warm and cordial welcome to all those who have honored us with their presence upon this occasion. It is a gratification to see so many of the citizens of the United States amongst us. Time has been when so large an influx from the other side of the frontier might have awakened other feelings, not, possibly, unmixed with some degree of apprehension. But, thank God, these feelings have passed away, and we can now unite in one common purpose, whether for the advancement of commerce or the promotion of science, feeling no other rivalship than that of a generous emulation. We have, many of us, enjoyed the hospitality of our neighbors on the other side. It is not for us to praise ourselves, but I trust that that virtue is not deficient on this side of the national frontier. Thanks to the benefactors of mankind, and the cultivators and promoters of science, we have ample means of conveying our thoughts and persons without much difficulty; and when that stupendous structure is completed which is striding across the river, and bids fair soon to afford the means of connection with both sides, I trust our communication will then be complete. I cannot conclude without expressing my regret at the absence of Professor Bailey, who had established for himself a European reputation, and whose presidency, upon this occasion, so many looked forward to with great pleasure. While we bow to that dispensation which has deprived us of his services upon this interesting occasion, I think we have, in Professor Caswell, a very worthy successor.

The President said he had much pleasure in introducing to the Association Professor Ramsay, who was here as the delegate from the Geological Society of Great Britain.

Professor Ramsay said it gave him great pleasure to find himself on this side of the Atlantic among his scientific friends, whether geologists, or those who devote their attention to other branches of science. He said scientific friends, because he had the honor and the pleasure of numbering many of those gentlemen among his friends. He had found, wherever he had gone, that there was a kind of brotherhood among scientific men, — that, go where you will, and merely announce your name, you are received with that affection, which is one of the greatest pleasures in scientific intercourse. He regretted that the gentleman first deputed by the Geological Society to attend this meeting, found he was not able to come. Sir Roderick Murchison was deputed, but his increasing years made him consider that it was advisable that he should not come, and the choice of the society then fell upon himself. He had no idea of coming, having made arrangements to go to the top of Mont Blanc, where he expected to derive the greatest possible pleasure; but being appointed to represent the society at this Association, he gave up that excursion, as he anticipated ten times more pleasure and ten times more instruction by coming to this meeting of the American Association.

The President said he had the pleasure of introducing Dr. Berthold Seemann as the delegate of the Linnæan Society of Britain.

Dr. Seemann expressed the great pleasure he had in being present, and in declaring the high esteem which the society he represented had always entertained for the American Association. The Linnæan Society had always taken a deep interest in this Association, and he had no doubt that this meeting would add another chapter to the proud history of American science. He begged to offer them the most sincere congratulations of the society he had the honor to represent.

The President said that Professor J. W. Dawson would now address them on behalf of the Natural History Society of Montreal.

Mr. Dawson said he did not intend to trouble them with saying any thing upon the present occasion; but, as his name had been placed upon the programme, he would take the opportunity to offer the Association a most hearty welcome to Montreal. He would not attempt to make any speech at present, but he hoped to have, on to-morrow evening, an opportunity of saying a few words, as it was proposed to invite all the members of the Association to an entertainment, which will enable them to have some social interchange of sentiment with one another. In behalf of the Natural History Society of Montreal, he would say that they most heartily rejoiced to see so large a representation of American science as that now present; and they hoped that this meeting would be very successful, — one which will give a strong and healthy stimulus to the pursuit of Natural History in this city and in Canada. If so, the Natural History Society will regard this as the greatest service which they have done to the cause of American science.

The Annual Address was delivered by the retiring President, Professor James Hall, on Tuesday afternoon, August 18.

On the afternoon of August 14, Dr. Hare delivered, in general session, his paper on "Travelling Whirlwinds."

On the evening of Friday, August 14, Dr. A. A. Gould, of Boston, offered a tribute to the memory and services of the late Professor J. W. Bailey, elected President of the Association for the Montreal Meeting. Professor D. Olmstead, of New Haven, followed in a similar tribute to the late William C. Redfield, the first President of the Association. These addresses are printed in this volume.

No lengthened abstract of the proceedings, scientific and executive, of the Montreal Meeting of the Association is necessary in this place, as they are contained in full in the papers and resolutions printed in this volume.

The Association voted to hold their next meeting at Baltimore, Maryland, on Wednesday, the 28th of April, 1858, having received an invitation to visit that city from the Maryland Historical Society and the Maryland Institute of the city of Baltimore.

The officers elected for the next meeting, are Professor JEFFRIES WYMAN, of Cambridge, President; Professor JOHN E. HOLBROOK, of Charleston, S. C., Vice-President; Professor WILLIAM CHAUVENET, of Annapolis, Md., General Secretary; and Dr. A. L. ELWYN, of Philadelphia, Treasurer. The Permanent Secretary, Professor JOSEPH LOVERING, of Cambridge, was elected, at Albany, for a second term.

During the session of the Association, the members and their ladies, besides receiving many private hospitalities, were elegantly entertained on Thursday evening, August 13, by the Natural History Society of Montreal; on Monday evening, August 17, by the Governors, Principal, and Fellows of McGill College; and on Thursday, August 20, by the Corporation of the City of Montreal. On Saturday afternoon, August 15, an excursion was made to St. Helen's Island, by invitation of Colonel Monro of the 39th Regiment; and on Thursday, August 20, an excursion was made, by invitation of the Local Committee, to St. Anne's and Beauharnois, in which an opportunity was afforded to examine the Potsdam limestone in that vicinity, and pass down the Lachine Rapids.

RESOLUTIONS ADOPTED.

Resolved, That a Sub-Committee of three be appointed to report to the Standing Committee such Rules and Regulations, of a permanent character, as may seem capable of promoting the regular despatch of business at future meetings of the Association.

Resolved, That a Preliminary Committee be appointed to inquire and ascertain what has been done upon the subject, in the several States, of the Registration of Births, Deaths, and Marriages, and report to the Standing Committee at their next meeting.

Resolved, That a Committee be appointed to wait upon the retiring President of the Association, and invite him to deliver his address at some proper time and place, and under more favorable circumstances than those which existed at the social meeting of August 13.

Resolved, That the retiring President be requested to notice in his address the services of members active in the advancement of science, who have died within the year.

Resolved, That the printing of the programmes for the remainder of this Meeting be placed under the superintendence of the Permanent Secretary, and that the Standing Committee of the Sections be called upon to deliver the programmes of their respective Sections and Subsections to the Permanent Secretary, the chairmen of the Sub-sections providing them with the means for this purpose.

Resolved, That a Committee be appointed to report on Dr. Hare's Storm-Curves.

Resolved, That two hundred and fifty extra copies of the Address of Professor Hall be printed, and presented to the author.

Resolved, That one hundred dollars be appropriated, according to the recommendation of the Permanent Secretary, to provide suitable accommodations for the property of the Association, amounting to nearly four thousand volumes.

Resolved, That no notice be taken, by title or otherwise, in the published Proceedings, of any article not approved by the Standing Committee.

Resolved, In accordance with the recommendation of the Physical Section, that a Committee be appointed on the Coast Survey (according to the request of the Superintendent), and that the books presented by the Superintendent be referred to this Committee.

VOTES OF THANKS.

Resolved, That the Association highly appreciate, and warmly recip rocate, the friendly sentiments expressed, at the opening of the session, by HIS EXCELLENCY, GENERAL SIR WILLIAM EYRE, K. C. B., Administrator of the Government.

Resolved, That the thanks of the Association be tendered to the CORPORATION AND CITIZENS OF MONTREAL, for their generous hospitality.

Resolved, That the thanks of the Association be returned to the LOCAL COMMITTEE, for their convenient arrangements for its accommodation, and for their unwearied exertions to promote the personal comfort of the members.

Resolved, That the Association tenders its thanks to the NATURAL HISTORY SOCIETY, for the elegant reception and entertainment given to the Association at Concert Hall.

Resolved, That the thanks of the Association be returned to the GOVERNORS, PRINCIPAL, AND FELLOWS OF MCGILL COLLEGE for the cordial reception and fraternal welcome extended to its members at Burnside Hall.

Resolved, That we recognize, with peculiar satisfaction, the presence of the eminent SCIENTIFIC GENTLEMEN FROM THE OLD WORLD, who have honored this meeting with their attendance.

Resolved, That the Association returns its grateful acknowledgments to the CORPORATION OF THE CITY OF NEW YORK, and to the ACADEMY OF SCIENCES OF THE CITY OF ST. LOUIS, for their polite invitations to hold our next meeting in their respective cities.

Also, to the JUDGES of the DISTRICT OF MONTREAL, and to the BAR, for the accommodation afforded to the Association in the use of the Court House.

Also, to the CANADIAN INSTITUTE, and to the MECHANICS' INSTITUTE, for civilities extended to the Association by these bodies respectively.

Resolved, That the thanks of the Association be tendered to the DIRECTORS of those Railroad and Steamboat Lines, on which free return tickets have been offered to members; and also to the DIRECTORS of those Lines of Ocean Steamers who have placed free passages from Europe at the disposal of the Local Committee.

Resolved, That the thanks of the Association are due to the MERCANTILE LIBRARY ASSOCIATION of Montreal, for affording the members the free use of their Reading Room, and tendering them complimentary tickets for the Exhibition of Paintings.

Resolved, That our cordial thanks be returned to Professor ALEXIS CASWELL, for the courteous, dignified, and efficient manner in which he has presided over the Association at its present meeting.

Resolved, That the thanks of the Association be presented to the PRESIDENT AND DIRECTORS OF THE BANK OF MONTREAL, for their liberality in negotiating the funds of the Association.

REPORT OF THE PERMANENT SECRETARY.

The Permanent Secretary submits his annual report to the Standing Committee as follows: —

The affairs of the Association under his charge since the Albany Meeting relate chiefly to the collection of assessments; to the making of contracts, and the payment of bills, involved in the publications of the Association; to the examination of papers presented at the Albany Meeting, the correction of proof-sheets, and their transmission to the authors of the papers; to the distribution of the Albany volume to members entitled to receive copies; to assisting in arrangements at the Albany Meeting, and in preparations for the Montreal Meeting; and, finally, to issuing to members of the Association the following classes of circulars: 1st, calling on members for a copy of their papers, read at Albany; 2d, notifying new members of their election; 3d, calling upon delinquents for their arrears; 4th, transmitting receipts for assessments received by mail; 5th, notifying members when the printed volume was ready; 6th, notifying members *when* and *how* this volume had been sent to them. To these duties may be added the general correspondence during the interval between the tenth and eleventh meetings of the Association. The number of members in the Association is now so large, and the published volume so much exceeds in size any previous one, that the duties of the Permanent Secretary have required much of his time and attention. And yet these duties are of such a nature, and the members of the Association hang together by so loose a tenure, that he can hardly suppose that, with all his pains, he has discharged them to the satisfaction of every one.

It cannot be expected of the Treasurer of the Association, who receives no compensation, that he should do more than hold in trust the unappropriated funds of the Association. It is proper, on this account, as it is far the most convenient arrangement, that the assessments should be paid to the Permanent Secretary, as the Constitution allows, and as is, in fact, the general practice. Since the 20th of August, 1856, when the Albany Meeting began, down to the 12th of August, 1857, when the Montreal Meeting commenced (including exactly a financial year), $2,363.34 have been collected by the Permanent Secretary, partly by the sale of the volumes of Proceedings, but principally in payment of the last annual assessment and of arrears.

This sum exceeds, by $226, that collected in the same way during the preceding year, although the latter was considered financially highly prosperous to the Association. To this sum must be added $10, received from A. D. Bache, in repayment of Peters's plate "On the Solar Spots," $19 received from G. P. Putnam & Co. for the sale of Proceedings, $30 drawn from the Treasurer, Dr. A. L. Elwyn, and a balance of $106.31 remaining, from the previous account, in the hands of the Secretary, the whole constituting a fund of $2,528.65. From this income have been expended $2,417.30, the items of which expenditure will be found particularly given in the account-book which accompanies this report, but which may be generally stated as follows:—

1. Expenses in the publication and distribution of the Albany volume,	$1,723.62
2. Salary of the Permanent Secretary,	500.00
3. General expenses, including postage, and charges incurred at the Albany Meeting, etc.,	193.68

There remains, on this account, in the hands of the Permanent Secretary, a balance of $111.35, which has been carried in the account-book to the credit of the Association. The balance in the hands of the Treasurer has also increased by $30; so that it now amounts to $821.70. If this sum is added to the balance in the hands of the Secretary, it makes a total of $933.05 to the credit of the Association.

The Permanent Secretary would state, in conclusion, that the property of the Association in his hands consists of 3,898 volumes of Proceedings. These volumes are stored away in boxes, and a recent examination has shown that they are suffering for want of air, and other more appropriate accommodations. He recommends an appropriation of $100, in order to place them on suitable shelves in a dry place. This plan will not involve any annual charge, and the expense of it will not exceed, probably, the sum received from the sales in a single year.

JOSEPH LOVERING,
Permanent Secretary.

MONTREAL, August 17, 1857.

CASH ACCOUNT OF THE

Dr. AMERICAN ASSOCIATION IN

Stationery for two years	$20.00
Munsell, for printing envelopes	9.50
Express to Albany	3.00
Horton, for services at Albany	16.00
Expenses in attending meeting	75.00
Bad bank-note	2.00
Rice & Kendall, for paper	299.52
Buck's Express	10.62
Stratton's Express	2.62
Kilbourn & Mallory, for wood-cuts	103.00
J. Bien, for printing 7 plates	112.50
Rice & Kendall, for paper for covers	6.90
Rice & Kendall, for paper	88.92
Allen & Farnham, for printing	356.81
Metcalf & Co., for printing	356.21
Gorham's bill for printing plate	15.25
Bradford, for printing plate	43.00
Mills, for services	15.00
Rice & Kendall, for paper	132.00
Higgins & Bradley, for binding	101.79
Buck's express	11.75
Index to Albany volume	5.00
Circulars to delinquents	15.00
Redfield, for plates	48.84
Express to Montreal	10.89
Postage and discount	56.18
Salary of Permanent Secretary	500.00
	$2,417.30
Balance to next account	111.35
	$2,528.65

PERMANENT SECRETARY.

Account with JOSEPH LOVERING. Cr.

Balance from last account	$106.31
Draft on Treasurer	30.00
Cash from G. P. Putnam & Co., for sale of Proceedings .	19.00
From A. D. Bache, for Dr. Peters's plate . .	10.00
Assessments, &c. (from No. 357 to 950) . . .	2,363.34
	$2,528.65

STOCK ACCOUNT OF THE PERMANENT SECRETARY.

Volumes Distributed or Sold.

VOLUMES	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
DELIVERED TO										
Boston City Library *					*	*	*	*	*	
Boston City Library										*
Patent Office *							*	*	*	*
Drowne *	*	*	*	*	*	*				
Drowne							*	*	*	
J. B. Smith *	*	*	*	*	*	*				
J. Munsell *	*	*	*	*	*					
Nantucket Athenæum †						*	*	*	*	*
Munroe & Co.‡										20
Westermann *		*								*
J. C. Teeley *										*
Dudley Observatory										*
Albany State Library										*
Phil. Acad. Nat. Science †										*
Phil. Philos. Soc. †										*
Providence Athenæum †										*
Brown University										*
Yale College										*
Harvard College										*
American Academy †										*
Boston Nat. Hist. Society										*
Boston Athenæum										*
Michigan University *										*
Super't Education, Montreal										*
Smithsonian Institution †										*
C. M. Blake							*	*	*	*
W. H. Davies							*	*	*	*
J. P. Doyle *										*
Wurtelle *										*
W. M. Roberts							*	*	*	
Local Committee at Montreal										*
C. M. Morse									*	*
W. D. Henkle *										*
A. K. Eaton										*
Dr. A. S. Baldwin										*
W. Baylis *										*
C. P. Treadwell										*
E. J. Horan										*
W. F. Phelps										*
W. C. Little										*
To Members	1				1		21	17	28	431
Total	4	4	3	3	5	4	28	24	36	484

* Sold.

† By order of the Association.

‡ In exchange for twenty copies of Vol. II.

BALANCE OF STOCK.

Volumes.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
Balance, March 20, 1857,	65	67	265	244	454	312	581	895	1015	
Received from Binders, .										1487
Received from Monroe & Co.,* . . .		20								
Total, ...	65	87	265	244	454	312	581	895	1015	1487
Delivered to Members or sold, ...	4	4	3	3	5	4	28	24	36	484
Balance, March, 1858, .	61	83	262	241	449	308	553	871	979	1003

* In exchange for volumes less rare.

ACCOUNT OF G. P. PUTNAM & CO. WITH THE ASSOCIATION.

Volumes on hand.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	Value.
January 1, 1857,	4	4	4	69	7	152	8	8	8	$395.84
January 1, 1858,	0	2	0	67	2	148	1	4	3	$352.95
Now due, for sold,	4	2	4	2	5	4	7	4	5	$ 42.89

March 1, 1858.

REPORT OF THE AUDITORS.

We hereby certify that we have examined the preceding account of the Permanent Secretary, comparing the credits with the Treasurer's account, and the receipt book and the debits with the several vouchers, and find the whole correct, and the balance of one hundred and eleven dollars and thirty-five cents properly credited in the next account.

Signed, Denison Olmsted, B. A. Gould, Jr., } *Auditors.*

REPORT OF THE TREASURER.

I have received from the Permanent Secretary a check on the Phœnix Bank of New York, for thirty dollars, and on the Charles River Bank a check for thirty dollars; from members in assessments, sixty dollars; in all, one hundred and twenty dollars. I have paid from the treasury Duval's bill (for lithographing), sixty dollars; a draft by Joseph Lovering, thirty dollars; in all, ninety dollars. The excess of receipts above expenditures (amounting to thirty dollars), added to balance of last year, which was seven hundred and ninety-one dollars and seventy cents ($791.70), makes the balance to next account eight hundred and twenty-one dollars and seventy cents ($821.70).

A. L. Elwyn, *Treasurer.*

CORRESPONDENCE.

West Point, January 27, 1857.

Dear Sir,—I have the honor to acknowledge with thanks the reception of your letter, informing me officially of my election to the Presidency of the American Association for the Albany Meeting.

As there is no prospect that my health will permit me to attend the meeting, I shall, in due time, send you a letter of thanks to the Society for the honor of the election, and regret that I cannot perform the duties of the office. I shall also give Prof. Caswell due notice.

Yours very respectfully,

J. W. Bailey, *Prof. of Chemistry.*

Prof. Lovering, Cambridge, Mass.

Baltimore, February 17, 1857.

Dear Sir,—At a meeting of the Maryland Historical Society, held in the month of January, a Resolution was passed inviting the American Association for the Advancement of Science to hold its meeting in 1858 in the city of Baltimore, and appointing a committee consisting of Lewis H. Steiner, Rev. J. G. Morris, D. D., and Rev. G. W. Burnap, D. D., to present the invitation at the Montreal Meeting.

I have deemed it proper thus early to let you know officially of this invitation,—and as this Society represents our best citizens, it insures a hearty welcome.

With sentiments of highest respect, yours, etc.,

Lewis H. Steiner, *Chairman of Committee.*

Prof. Jos. Lovering, *Per. Sec'y Amer. Association.*

Maryland Historical Society's Rooms, Baltimore, January 8, 1857.

At a regular meeting of this Society, held on this evening, the following resolution, offered by Dr. Lewis H. Steiner, was unanimously adopted: "*Resolved*, That an invitation be extended by the Maryland Historical Society to the American Association for the Advancement of Science, at its meeting at Montreal, on the 12th of August next, to hold the meeting of the said Association in 1858 in the city of Baltimore, — and that the President be empowered to appoint a committee to present the invitation at the Montreal Meeting, and to express the earnest hope that it may be accepted."

Upon the adoption of the resolution, Dr. Lewis H. Steiner, Rev. Dr. J. G. Morris, and Rev. Dr. George W. Burnap were appointed to serve as the Committee.

J. SPEAR SMITH, *President.*
F. STREETER, *Secretary M. H. S.*

BALTIMORE, July 31, 1857.

GENTLEMEN, — The Maryland Institute of this city, established for the promotion of certain branches of science, whose universal cultivation and advancement form the object of your Association, have conferred upon the undersigned the honor of appointing them a committee to request that you will hold your next meeting in Baltimore.

The members of the Institute, with unanimity, displayed great solicitude that this request should be favorably entertained; but they have left to us, as more becoming, to present, in our discretion, the motives that are to be considered and may be most efficacious in influencing the acceptance of the invitation.

Among such motives, then, appearing to us entitled to great weight, are the centrality and accessibility of this city, more than of any other place where your meetings have been, or can be, held; the convenience of transportation of members of your Association to and from the meeting, which, through the agency of the Baltimore and Ohio Railroad, will be made less troublesome and more extensive than has been the case hitherto; the adaptation of the various apartments of the Institute, unrivalled, we believe, in America, either for the inaugural and valedictory assemblages of all your members, as well as of a numerous auditory which it is reasonable to suppose will be stimulated and improved by witnessing your proceedings, or for the separate meetings of different sections of your Association for the working prosecution of their respective subjects.

These motives have more especially a corporate regard. As to what concerns the Association more particularly in regard to its individual composition, we believe we say enough in adding that Baltimore has, in more than one instance, shown her appreciation of the presence of distinguished and philanthropic persons, and that in no instance has she been considered behindhand in the exhibition of a genial hospitality.

As to the period of holding your meeting, thus invited, the Institute appears to have thought it more respectful, instead of fixing any time, to leave it to you to determine the season and date most convenient to yourselves.

We sincerely hope, Gentlemen, that the suggestions we have had the honor of making will be acceptable to you; and that you will, before your adjournment, authorize us to report to our Principals the agreeable intelligence of your willingness to accept their invitation, and also of the precise date, against which preparations are to be made for your suitable reception.

And we have the honor to remain, Gentlemen,

CAMPBELL MORFIT.
J. H. ALEXANDER.
A. D. BACHE.

To the American Association for the Advancement of Science, now at Montreal, Canada East.

MAYOR'S OFFICE, New York, August 11, 1857.

DEAR SIR, — In behalf of the Corporation of this city, I beg leave, through you, to invite the American Association for the Advancement of Science, to hold their next Annual Meeting in this city.

It will give us pleasure to meet so distinguished a body of savans.

Very truly,
FERNANDO WOOD, *Mayor*.

Dr. L. FEUCHTWANGER.

The Academy of Science at St. Louis herewith respectfully tender to the American Association for the Advancement of Science a cordial invitation to hold its next Annual Meeting in the city of St. Louis.

As President of the Academy, permit me to assure the Association of an earnest and hearty welcome, should it consent to honor St. Louis with its presence.

With much respect, etc.,
B. F. SHUMARD, *Pres't Acad. Sci. St. Louis.*

To the American Association for the Advancement of Science.

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T.

U.

V.

W.

Z.

ERRATA.

PART I.

Page 67, line 4 from bottom, *for* Q' *read* Q.
" 69, in Formula, *for* A *read* a.
" 75, line 4, *for* any *read* any other.
" 75, " 17, *for* less *read* greater.
" 77, " 10, *for* invariable law *read* law.
" 78, " 3, *for* v *read* v^2.
" 79, " 11, *for* $\frac{D_{x-n}}{D'_x}$ *read* $\frac{D_{x-n}}{D_x}$.
" 81, " 4, *omit* or S^v_x
" 82, " 4, " x.

PART II.

Page 27, line 4 from bottom, *for* feet *read* furlongs.
" 31, " 5 from top, *for* Hupan *read* Huppan.
" " " 8 from bottom, for feruginious *read* ferruginous.
" " " 11 from bottom, *for* woolly *read* woody.
" 36, " 10 from top, *for* linneus *read* limneus.
" 44, " 8 from bottom, *for* leave *read* leaves.
" 45, bottom line, *for* pale-red *read* pale sea.
" 65, line 18, *for* Temiscaming *read* Temiscamang.
" 46, " 22, *after* area *insert* of azoic rocks.
" 46, " 7, *for* Shibahahnahning *read* Shebahahnahning.
" 152, " 4, *for* where *read* when.

ERRATA

In the Volume of the Proceedings for 1856, Part I.

Page 85, line 5, *for* C *read* D.
" 87, " 23, *after* $\Sigma_x^{x+n} S^v_x$ *supply* $-\frac{S'_x}{2}+\frac{L_x v^x}{2}$.